TEXTBOOKS ON PSYCHOLOGY

General Psychology. 2ND EDITION. BY *J. P. Guilford.*

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Theoretical Foundations of Psychology. EDITED BY *Harry Helson.*

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THEORETICAL FOUNDATIONS OF PSYCHOLOGY

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FOREWORD

When Professor Helson was approached with the suggestion that he prepare for this Psychology Series a manuscript on the schools or points of view in psychology, he was not long in making a counter-suggestion. It became clear to him, as it must to all thinking psychologists, that we have passed beyond the school-forming era and that lines of affiliation are no longer clearly drawn. Yet, most of the theoretical issues that gave rise to schools are always with us Furthermore, the broadening of psychology, into the fields of learning and motivation and the growing emphases upon personality and psychotherapy, particularly, have served to multiply those issues. At the same time, developments in modern philosophy of science have given psychologists new ways of looking at many of the issues.

Professor Helson's proposal was to approach the subject from the frame of reference of fields of psychological investigation and of problems and to let the schools speak where they have something to offer. It will be seen that, as it turned out, only infrequent reference needed to be made to traditional points of view. The development of psychology and the growth of issues have left them far behind.

It was another happy thought of Dr. Helson's to have the volume written as a symposium His invited authors generally rose to the occasion and have written as if they enjoyed their assignments and as if they extended their own thinking to meet new requirements. It is hoped that these new presentations will serve both the science and profession of psychology and the students who are preparing themselves to join the ranks of psychologists

J. P. GUILFORD

PREFACE

There are many books written on many aspects of psychology: books on general psychology, abnormal psychology, child psychology, tests, history of psychology, experimental psychology, methods and measurements in psychology. This book is designed to treat the fundamental ideas, concepts, theories and problems which are at the center of the chief divisions of the subject Consideration of the theoretical foundations of an experimental science, as psychology is today, cannot be divorced from concrete facts or basic procedures. There is a delicate interplay of fact and theory in the best scientific work. Empiricism guided by concepts leads to fruitful theories Worthwhile theories lead to concrete data which attest their validity and bring to light new facts. Hence the reader will find in this book on the theoretical foundations of psychology much discussion of facts techniques, methods, and experimental data relevant to theoretical constructs. Since facts and theories must often be seen in the light of their historical antecedents to be understood and evaluated, the reader will also find numerous references to history and schools of psychology and important contributions of workers in various fields

In planning this volume the editor had hoped to have the main fields of psychology each represented by at least one chapter, and to obtain fairly complete coverage of each field within each chapter That this hope has not been fulfilled is not the fault of either the editor or the contributors. A subject as vast and variegated as present-day psychology cannot be entirely covered, even in its theoretical aspects, in a single volume A number of important fields, for example, Individual Differences, had to be omitted entirely, and not all of the important concepts have been included in the treatments of the fields represented in this book This is due not only to the limitations of space, but also to the interests of the editor and of the contributors. Each contributor was given *carte blanche* to organize his field as he wished, so long as it served the main purpose of the book. Selection was therefore inevitable and indeed desirable; who would wish to include every idea on a given topic? Figure-ground relationships are as important in delineating concepts and theories as they are in perceiving configurations Although, therefore, *complete* coverage has

not been obtained either of psychology as a whole or of its particular fields, a sufficient amount of material has been presented to give the student a head start on many of the basic problems.

It is hoped that students will find this book of value in deepening and broadening their view of present-day psychology. This volume should help advanced undergraduate and graduate students of psychology to achieve some measure of integration of particular fields as well as of psychology as a whole. Whether they do this by accepting the point of view and organization of the contributors to this volume, or by reacting negatively toward particular expositions, is of minor importance. Either way the student will make progress and the purpose of the book will be realized.

This book should be of value also to the mature psychologist in so far as no one individual is able today to keep up with even basic considerations in the many fields comprising scientific psychology. The mature psychologist, familiar with the best thinking in his own field, will welcome, we hope, basic treatments of other fields.

The editor must first of all acknowledge the splendid co-operation of the contributors who, with a minimum of instruction and in most cases with no personal meetings or discussions, have succeeded in producing a volume fairly homogeneous as to level and type of material. A number of colleagues besides the contributors have helped to make the task of editing this volume easier for the editor by reading certain portions of it, and I am grateful particularly to Quinn McNemar, Evelyn Raskin, David Raab and Pearl Bretnall Meissner on this score. Thanks are also due to the several publishing houses, societies and associations who have given permission to include quoted material and figures and tables in the book. The final stages of the book, so prosaic but so important, were considerably lightened through the help given by Evelyn Raskin and my wife, Lida Helson, with the indices, and by Hannah B. Pell in various ways, especially with the preparation of manuscripts I cannot end this preface without thanking J. P. Guilford, the general editor of the Van Nostrand Psychological Series, who provided the inspiration for this undertaking.

H. H.

CONTENTS

CHAPTER 1

METHODOLOGICAL CONSIDERATIONS

By MALCOLM G. PRESTON, *University of Pennsylvania*

CHAPTER SUMMARY

Chapter 1 enumerates and discusses the methods used by psychologists in creating knowledge The early part of the chapter distinguishes between methods used in the *formation* of hypotheses and methods used in the *testing* of hypotheses. The principles underlying methods of both kinds have very general application; they are evident over the various branches of science, as well as the various branches of psychology. On this account, the chapter draws its illustrations from the sciences generally, and deals to some extent with problems in epistemology and the logic of science. This is all to the good, since it is well that students of psychology realize the essential unity which embraces all of the sciences, as well as the several parts of psychology.

Unity is often created by the existence of common problems Among the common problems faced by the sciences, and the various branches of psychology, are those which are due to the necessity of defining concepts, to the clarification of what is meant by certain crucial concepts, like the concept of definition itself, as well as the problems created by the often-mentioned relationships existing among the various sciences. In addition are the problems that come into focus when the student reflects upon the definition of the experimental method, and the emerging realization of the close relationship which exists between experimental and statistical methods. Indeed, it often appears that the relationships between these two methods have now achieved a status in which it is very difficult to define the one without reference to the other.

Neither the unity of the sciences nor the unity of psychology as a science, which are the central themes of the chapter, are clearly

apparent on the surface of things. The obvious differences in concepts and subject matter, which are so characteristic of knowledge, as it relates to the atom and to man, are very compelling.

Knowledge and Methods

Authority Is No Defense to Serious Criticism of a Psychological Theory.—Certain kinds of defenses ordinarily count little against serious criticisms of a psychological theory. For example, the argument that certain facts or conclusions or theories have been accepted by authority, while usually accepted as a strong argument by the general public, or by beginning students, is not regarded as being relevant where the criticism goes to the basis of a scientific theory. In such a context, argument on the basis of authority has merely a historical significance. It reveals who formed the conclusion; it does not reveal the essential matter, namely, the grounds upon which it was held.

A special form of defense to criticism on the basis of authority is the resort to special experience. This kind of defense is often encountered when questions are raised in areas where methods are relatively unstandardized, or in which the materials are so remote that the observations cannot be repeated. For example, in the early days of clinical psychology, methods were in a relatively primitive state of development, and theory was necessarily based upon conclusions formed from purely qualitative observations made over a series of cases by an experienced clinical psychologist. Often it was based on what the clinical psychologist called a "typical case" The general inaccessibility of the materials of the cultural anthropologist resulted, in the same way, in weight sometimes being attached to the experience of the observer in the early days of that science.[1] But, however necessary to the making of a beginning in a new area of knowledge, or to the suggestion of new hypotheses which are worthy of careful investigation, special experience is, like authority, a weak answer to a criticism of a scientific conclusion.

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[1] For an account of the objectivity of methods in modern cultural anthropology, see Mead, 1946.

Serious Criticism Is Answered by an Analysis of Methods.—The strongest criticisms which can be made of a scientific theory are those which attack the methods used, either in the development of the theory, or in its testing. Such criticisms, when successful, show inconsistency at one or more of four levels. These are: (1) the definition of the concepts, (2) the logic with which such concepts are used, (3) the suitability of the test of the theory to the requirements of the theory, and (4) the conduct of the test of the theory. It follows that the best defense against such criticism is the searching analysis of the methods used at each of these four levels. Scientists recognize this fact by devoting great energies to the study of the properties of such methods, the range of conditions under which they are applicable, the special limitations to which they are subject, and the extent to which knowledge is a function of the method used. No doubt much of the achievement of modern science, and much of its enormous prestige, is due to this incessant preoccupation with the methods by means of which scientific knowledge is made.

Criticisms of Methods Possess Various Degrees of Generality.—Criticisms of theories which are based on an analysis of the methods used possess various degrees of generality. Least general are criticisms of the conduct of the test of the theory. Such criticisms are leveled at the techniques which have been used in the determination of the facts that test the theory, and hence they hold for only a narrow class of experiments. For example, theories concerned with the PGR (the psychogalvanic response) and the muscle-action potential have sometimes been attacked (Woodworth 1938) by such criticism. The determination of the PGR and the muscle-action potentials is subject to difficulties of various kinds, most of which reflect the essential difficulty in the measuring of electrical quantities (like resistance) in the cutaneous or subcutaneous tissues, under known conditions. Another illustration of this principle is found in the debate that ensued during the early years of the study of the electrical potentials of the auditory nerve (Boring, 1942), over the question whether the quantities under observation were to be referred to the auditory nerve or to the cochlea. The

issue was serious, since its determination had a bearing upon important theories of the neurological basis of audition, but, again, it affected only experiments in a narrow class.

Somewhat more general than a criticism of technique, but still of the same kind, is a criticism of the applicability of an experiment to a proposed theory. Such a criticism may show that the facts as reported may be placed in more than one context, the investigation failing to distinguish between the contexts. For example (Stone, 1929, 1; 1929, *2*), it is known that rats of different ages require diets of different quantities, the younger rats requiring larger diets to maintain normal weight and activity. In experiments upon the effects of age upon learning ability, it is essential to the success of the experiment that the animals be maintained upon a diet carefully calculated to produce an equal strength of the hunger drive in rats of all ages. It is fatal to this requirement if all rats are fed the same diet. If the diet is substandard for young growing rats, it may in all likelihood be above standard for old rats; if it is substandard for old rats, it will evoke much stronger hunger drives in the young animals. Experiments on the effect of age upon learning in which young and old animals are maintained on the same diet have been criticized successfully by showing that their results are as relevant to the study of the physiological drives, as they are to the study of age and learning.

At the other extreme of generality, in the kind of criticism which can be applied to psychological theories, are attacks on the concepts used in the investigation. Such criticism has a general impact; it attacks the structure in which the facts are embedded, rather than the facts themselves. For example, auditory theorists (Wever and Bray, 1933) raise the question whether measurements of electrical processes in the cochlea or in the auditory nerve throw light upon hearing, when by hearing we mean the perception of sound Such a question becomes crucial, for example, when we wish to determine whether an insect can hear high pitches using only data on the action potentials taken off the auditory nerve.

Methods Are of Two Kinds.—The foregoing discussion suggests that a more fundamental distinction can be made among meth-

ods which lead to scientific knowledge than the distinction which has to do with the generality of their applicability. Methods are evidently used in determining questions of fact. Such methods are called techniques as they become more and more specific in their applicability. But methods are also used in the activity involved in choosing and framing the questions which, if not asked explicitly, are asked implicitly whenever a fact is determined. Such methods have to do with the development of hypotheses. Since they have to do with the definition of knowledge itself, we must expect to have to deal with problems in philosophy, particularly those parts of philosophy called epistemology and the logic of science, as well as problems in science, in general, and in psychology, in particular, if we are to make a complete survey of the methods that lead to psychological knowledge.

From the Point of View of the Scientist, What Is Meant by Knowledge?—The scientist must distinguish knowledge from perception. Perception refers to the impression that is formed by the organism on the basis of the various energies to which it is exposed. All students of perception are in agreement that it has to do with appearances. Knowledge goes beyond perception in that it detects uniformities that escape the processes of perception, and discloses differences where perception reports identities. On this account it is often said that knowledge has to do with realities. Important properties of knowledge are: (1) While it is created by a scientist, it must be independent of the scientist. This property suggests a corollary: anything which purports to be knowledge is verifiable by someone other than its discoverer. (2) Knowledge always depends upon systematic observation as contrasted to casual observation. (3) Knowledge possesses a degree of logical organization, and in this organization, concepts play a central role. These concepts pass by different names. Sometimes they are referred to as concepts and sometimes as logical constructs. (4) As knowledge has accumulated, its organization has given rise to the conception of "a model of nature."

Since the time of Kant it has been very common among philosophers and scientists (many of whom, it must be admitted, do not

agree as to what is meant by such fundamental concepts as appearance and reality) to distinguish between nature as it may exist, and that organized conception of nature which, reflected in the work of scientists of all fields, has come to be known as "our model of nature." It may be very surprising to realize the implications of the preceding sentence. If nature as it really exists must be distinguished from our organized conception of nature, clearly science must answer whether the methods of science are capable of drawing a final model. And, of more practical importance, if we make the distinction between nature as it may exist, and organized conceptions of nature, this implies an endless competition among conceptions. As to the first conclusion, science and philosophy are in general united in the conviction that science will produce no final model. As to the second conclusion, science in general, and psychology in particular, offer many illustrations of competing conceptions. Physics offers the competition between the Ptolemaic and Copernican theories, as well as that between the Newtonian and Einsteinian theories. Psychology offers the competition between the learning theories of Thorndike and Hull, on the one side, with those of Koehler and Wertheimer on the other.

Different "models of nature" often depend upon differences in fundamental conceptions as to the character of the natural universe. Perhaps the most striking distinction exists between those who believe that there is one natural universe and those who believe that there are two natural universes Those who believe that there is one natural universe are called monists, and those who believe that there are two natural universes are called dualists. Dualists distinguish between the physical universe on the one side, and the psychological universe on the other. The most common distinction made by the dualists is the distinction between mind and body. If such a distinction is made, an important problem is thereby created—namely, the problem of accounting for the apparent relationship between mind and matter. This problem takes many forms. It is raised, for example, whenever a question is asked as to the precise dependence existing between events in the physical universe, and events in our conscious response to stimulation by energy from that

universe. It is also raised whenever a question is asked as to how events in the nervous system are to be regarded as being related to psychological processes, of which the conscious processes are perhaps the best example.

The monists do not escape serious questions by their denial of the dualistic position. Those of them who are materialists have had to grapple with the problem as to how life and consciousness are to be placed in their model of a monistic universe. They have offered various solutions. Some have denied the existence of life and consciousness as events which are capable of scientific investigation (Watson, 1924). Others have endeavored to show that a change in the method of defining is necessary whenever we raise questions of a biological or psychological nature (Singer, 1924).

Postulates Play a Significant Role in Structuring Models of the Universe.—The student may well wonder how one can take a position on a question which appears to be a question of fact without having the facts available Nevertheless, such positions must be taken if any progress is to be made. All scientific work is based on elements which are taken for granted as well as elements of knowledge. When an assertion is made on the basis of faith it is called a postulate. It may be accepted because it is self-evident, or because its contrary is unthinkable. It may also be accepted provisionally, as a mere working basis. The important role played by postulates in structuring models of the universe is seen clearly in the physical sciences as a result of certain developments in pure mathematics and in physics. Conventional algebra (Birkhoff and MacLane, 1946) is based on several postulates of which one is the well-known commutative law which states that the sum or product of two numbers is the same no matter what the order in which they enter the operation. However strange it may seem, we now know that other algebras are possible in which the commutative law does not hold. One such algebra, the algebra of matrices, is used extensively by psychologists in the study of ability, personality, and attitudes.

Postulates have a history of implicitness; they are not always made explicit, and often an extraordinary intellect is required to

detect their existence (Wertheimer, 1945), and hence to submit them to rational analysis. Such have been the intellects in mathematics who have perceived the possibility of challenging such postulates as the axiom of parallels and the commutative law, and such an intellect was Albert Einstein, who first perceived that velocities of particles which are moving in universes which themselves are moving cannot be obtained by simple addition of the constituent velocities. As a consequence of the long tradition in the history of mathematics, as well as the work of Einstein, all science has learned that postulates must be made explicit. Psychology is no exception to this rule. Prominent among contemporary theorists who have paid close attention to the explicitness of postulates is Clark Hull, who has developed a theory of learning in which an effort has been made to use no postulates unless they were explicit.

Some postulates have all the properties of a definition (Churchman, 1948); such a postulate is the celebrated fifth axiom of Euclid which states that the corresponding angles formed by the intersection of a transversal with parallel lines are equal Other postulates have an experimental basis; they are capable of being tested and may be accepted or rejected on the basis of the test. Such postulates are often called assumptions. Wherever they are present in the design of a procedure which purports to provide a test of a theory, provision should be made for the accumulation of evidence so that an opinion can be formed as to whether or not they are justifiable. A common assumption made in many kinds of experimental work is that the sample of subjects to be used in the investigation conforms to some well-established principle It may be assumed that they follow the normal distribution curve, or that regression equations calculated from data which they furnish will be linear equations. Such assumptions should not only be made explicit, but experiments should also be formulated in such a way as to make express provision for their testing.

Some Knowledge Is Systematic; It Stands in a Defined Relation to Other Knowledge. Some Knowledge Is Unsystematic; It Is Unrelated to Other Knowledge.—No matter whether he is a monist or a dualist, within the limits of the universe

in which the scientist works, his motive, as a scientist, is not only to acquire but it is also to organize his knowledge in order to provide an intelligible account of the events which occur in that universe. In the process of developing this intelligible account, given bits of knowledge play very different functions depending upon the condition of the structure into which they are fitted. Occasionally a single observation is made which is seen to fit into organized knowledge only in such a way as to require a reorganization of existing theory. Such observations stand out as landmarks in the history of scientific knowledge. The same amount of work put into another bit of knowledge may do nothing more than settle the immediate question to which the investigative work was directed.

The physical sciences are well advanced, so that at the present time it is possible to offer many illustrations of instances in which the determination of a single fact— for example, the extent of the displacement of the image on a photographic negative—has been sufficient to cause a revolution in our conception of physical nature Psychology is not yet sufficiently advanced to offer many illustrations of this fact, but it is not without such landmarks.

In 1912 Wertheimer showed that subjects reported movement if stationary lines were presented in alternation, with intervals of approximately 60 milliseconds separating the successive exposures (Wertheimer, 1912). Such movement is well known today as "apparent movement" and forms the basis of the movement seen in motion pictures. In 1912 the dominant view in the field of perception rested on the assumption that the data of perception consisted in sensory elements These sensory elements were believed to be aroused by various forms of physical energy. Wertheimer demonstrated that the movement generated in his experiment had no counterpart among the sensory elements nor in the distribution of the stimulus energies. His facts could not be fitted into the existing theory of perception; hence a general reconstruction of that theory was required. The new theory of perception in turn affected the structure of theories of memory, thinking, judgment, and action, largely by provoking the definition of novel concepts, so that forty years afterward, Wertheimer's original work looms up as an important landmark in scientific history.

The Creation of Systematic Knowledge Has Often Been Related to the Hierarchical Order of the Sciences.—No matter how we approach the sciences, it is evident that they are not isolated attempts to understand unrelated aspects of nature. They stand in definite relationships to each other. In a fundamental position are mathematics and physics. Sometimes this position is assigned them because mathematical and physical techniques are very widely used. There is, however, another sense in which these two branches of knowledge are more basic than others Many scientists believe that the fundamental concepts of the remaining sciences will, under sufficiently intensive investigation, be shown to be nothing more than complex functions of mathematical and physical concepts. A classic illustration of a realization of this belief is the demonstration that the temperature of a substance is the mean velocity of its molecules.

It is a peculiarity, however, of some of the sciences, that they use concepts which cannot, at least as yet, be stated in the concepts of these basic sciences Biology uses the concept of life, and psychology uses the concept of behavior, neither of which has been defined successfully as functions of the variables of physics and mathematics.

Those psychologists who deal with a psychological problem in such a way as to explain their facts by showing them to be a function of laws, which themselves are in some known relationship to physical laws, are known as reductionists The reductionists have been very prominent in certain of the fields of psychology, notably action, perception, and learning. For example, in the study of perception, Boring (1933) has attempted to relate intensity, quality, extensity, and duration of experience to well defined properties of the physicochemical operation of the various sections of the nervous system. In the field of physiological psychology, much work (Fulton, 1943) has gone into the study of the dependence of coordinated action upon the properties of nerve tissue, synaptic connections between nerves, and muscle tissue. Some of this work has been very noteworthy because it has succeeded in showing that reactions which appear on the surface to be highly psychological, i.e., they are fluidly adaptive, appearing on the surface to be a product of

reflective thinking or at least to be motivated, are, as a matter of fact, a consequence of relatively simple properties of nerve and muscle tissue. Much of the study of unlearned behavior (instinctive behavior, as it is sometimes called), particularly in the invertebrates, has lent support to the reductionist's principle.

Another viewpoint on the status of the concepts of psychology holds that, while such concepts must be consistent with the concepts of other more basic sciences, they need not necessarily be reducible to them. Such a view supposes that nature can be discussed in more than one universe of discourse and that questions which are meaningful in one universe will be meaningless in another. This view finds support in the view taken by the logician who points out that it is possible to raise questions concerning a class of objects which are highly meaningful when referred to the function of the class as a whole, but which are meaningless when raised in connection with the mechanical properties of any single member of the class. Such questions have to do with the extent to which the members of the class realize their function (Churchman, 1948). For example, if we consider a class composed of watches, grandfather's clocks, sundials, and ringed candles (a class of instruments known as timepieces), one can raise questions of fact about the class as a whole which can be answered only by specification of the conditions under which the members of the class are to be observed. One such question is "Are expensive timepieces good timepieces?" The answer to this question cannot be given until the function of the timepiece is specified. If the watch is a present for a small boy it will sometimes turn out that the expensive timepiece is not the good timepiece. It appears that the same class object may be either a good or a poor timepiece depending on external circumstances.

It follows that statements about members of functional classes have a certain indeterminacy, the indeterminacy being an essential characteristic of the statements. This kind of indeterminacy is different from the celebrated indeterminacy associated with the name of Heisenberg. The Heisenberg principle of indeterminacy, which caused a tremendous revolution in the thinking of physicists, states that in the case of certain particles it is impossible to determine both position and velocity simultaneously because of the effect upon the

particle exerted by the photon of light by means of which the observation is made possible. The indeterminacy of radical empiricism, on the other hand, is due to the fact that its questions deal with systems in which statements are made about objects in a class where the class is defined in the light of its function and purpose. If any aspect of an experimental situation is indeterminate, it follows that such situations must be studied by statistical methods.

The Concepts of the Nonreductionists.—Reductionists regard the important objective of their science to be the explanation of their data in the language of sciences of a more fundamental character. This view may be held so strongly as to exclude any other kind of work as being scientific in nature. On the other hand, there are psychologists who while agreeing that psychological theory cannot violate the conceptions of the more fundamental sciences, are still little concerned about the precise relationships of their concepts to the concepts of the fundamental sciences Such people, of whom Kurt Lewin is a good example, believe that the most fruitful mission which psychology can accept is the working out of the relationships of the concepts within the science (Lewin, 1938), without regard to whether they will or will not ultimately reduce to the concepts of other sciences With such different views present among psychological theorists, it is not surprising that students of psychology become confused by the differences in the types of theories with which psychology abounds. Some of this confusion can be reduced by considering the theoretical structure of existing psychology as lying in a two dimensional space. While it does not always hold true, it is often the case that examination of relationships through the vertical dimension of this structure implies the reductionist type of theory, while examination of the relationships over the horizontal dimension implies the nonreductionist type of theory. It is certainly true that both types of theorist define their task as the investigation of the functional relationships existing among their concepts.

Reductionism in the Social Sciences.—In his analysis of the economics of consumption, Veblen notes the universal tendency for social classes to emerge in which great stress is placed upon the

consumption of goods according to the code that much is to be consumed toward no useful economic end (Veblen, 1934). Such classes consume conspicuously, and the evidence of the fact that the consumption is directed toward no useful economic end can be seen in dress, eating and drinking, architecture, the use of leisure, the material accompaniments of religious ritual, and many other aspects of social living. In the course of his development of the problem of conspicuous consumption, Veblen dealt with the problem of the origin of such a phenomenon. He believed that it had a psychological origin, that in fact it was a consequence of individual differences among the members of the human race in the strength of certain instincts, in particular the *predatory* instinct. This use of psychological concepts to explain an economic phenomenon illustrates reductionism at a level in the structure of the sciences higher than that usually assigned to psychology. As is true with psychologists, the social scientists are affected by the anxieties associated with the problem of reductionism The political scientists, economists, and the sociologists are concerned as to whether all of their problems will be solved when they have accounted for social institutions by giving an accurate account of the behavior of people relative to those institutions. Many of the social scientists are, like Veblen, reductionists; some however, fewer in number than those in the reductionist camp, are governed by the conclusion that the phenomena of the social sciences must always stand in a consistent relationship with the facts of psychology, but that some questions, peculiar to their own field, can be answered only by showing the interrelationships of concepts in the field and not by attempting a reduction to the concepts of psychology.

Operationism and the Definition of Concepts.—Related to the question of reductionism is the question of what is meant by the definition of a concept The fundamental concepts of physics are usually said to be time, space, mass, and energy. Since the work of Einstein, it has been recognized that mass and energy are equivalent, and that a satisfactory account can be given of the physical universe if measurement is made of three quantities, i.e., distance, time, and either mass or energy. These quantities are often said to be unde-

fined quantities, in the sense that they cannot be expressed as functions of any other quantity.

Any observations in which these quantities enter must of necessity be expressed in a unit, and in all three cases the conventional unit is arbitrary, it being well-known that the unit of distance is a certain separation on a platinum bar maintained at a certain place, etc. While it has been conventional to suppose that we can define such a thing as distance as depending ultimately upon the separation existing on a certain platinum bar, questions arise whenever distance is determined in two different ways. For example, distance may be determined by laying a ruler along an object, or by the process of triangulation, in the case of which the distance is a certain function of a measured angle. Whenever two measurements of this kind do not agree, the practical question arises as to the importance of the disagreement. A consideration of this practical question leads ultimately to a very fundamental question, namely: Can the concept distance be given unique definition?

An influential view in this matter was developed by Bridgman, who has remarked that the operations carried out in the making of a measurement are all that we have to support an assertion as to the meaning of the measurement On this argument, he has developed his theory of operational definition, which asserts that the definition of a concept is given by an account of the operations involved in the making of a relevant observation. According to such a view, there is not one definition of distance (Bridgman, 1945), but there are as many definitions as there are methods of measuring distance. Hence there is triangulated distance as well as ship's-log distance, tape-measure distance, speedometer distance, etc.

It is very easy to see that differences in these distances in any given case may be due to the presence of constant errors in the measurements. For example, speedometer distance in a certain case may be suspect if we discover that the diameter of the tire on the automobile is less than the diameter specified by the person who constructed the speedometer. However, there is still the problem as to the grounds on which we will conclude that two definitions of distance are equivalent (Israel, 1945) when we suspect that a dif-

ference between measures is due to random as against constant errors. Such differences between measurements will always exist; for this reason the question is not idle. It has also been noted that the adoption of operational definition does not rid science of absolutes completely, since such a step leaves the definition of an *operation* unstated (Israel, 1945).

Whatever our views on the identification of the operations with the definition, it is generally agreed that no definition is meaningful unless first one can find objects or classes of objects required for the use of the definition, and second, can carry out the necessary operations for the study of the objects or classes (Feigl, 1945). In other words, no question is a question of fact unless it can be answered by the use of specified operations or, as it has been put by Singer (Singer, 1930), by an experiment.

Experimental Variables Are Either Dependent or Independent Variables.—Irrespective of whether an investigator is a reductionist or a nonreductionist, or whether or not he believes that operations *define* concepts, the testing of his theory always requires him to see whether a given event is affected by a change in the conditions under which he observes it. The experimental condition which is often changed at the experimenter's will is usually called the independent variable. The event which is studied as the independent variable changes is usually called the dependent variable. No matter whether the independent variable changes continuously, stepwise or by qualitative variation, or whether the dependent variable changes in any one of these three ways, careful analysis of any experimental test of a theory will always permit the construction of a graph in which a geometric interpretation can be given to the results of the investigation. On such graphs, it is conventional to have the dependent variable on the ordinate, and the independent variable on the abscissa.

These independent variables are frequently defined in the physical universe. Thus, perceived auditory intensity is often reported as a function of *sound pressure* (Wever, 1948), as is perceived brightness as a function of *luminous flux per square meter of reflecting surface* (Woodworth, 1938). In the study of learning, change in

behavior is reported as a function of *number of repetitions* or of *exposure time* or of *number of reinforcements*. Dependent variables are also often defined in the physical universe. Thus amount of contraction of a muscle (measured in millimeters) will be the dependent variable in a conditioning experiment, while reaction-time, measured in milliseconds, will be the dependent variable in an experiment on preparatory set.

Some Dependent Variables Are Called "Intervening Variables."—The dependent variables which we have described in the last paragraph all have the property of being directly accessible to observation and measurement. Science also uses dependent variables which are not directly accessible to observation, but which must be studied by indirection. In psychology such variables include habit strength as defined and studied by Hull (Hull, 1943), "g" as defined and studied by Spearman (Spearman, 1937), inhibition as defined and studied by Pavlov (Hilgard and Marquis, 1940), and psychological force as defined and studied by Lewin (Lewin, 1938). Such variables are often called "intervening variables" because they are presumed to exist, in a logical sense, between the observable independent and dependent variables.

In psychology, the intervening variable can be known only by study of the behavior of the organism, and on that account it is often said that such variables are defined on the basis of behavior. They often become necessary in order to account for disparity between the conditions of stimulation on the one side, and the character of the response on the other side. In certain cases, this amounts to taking account of the fact that response is not a linear function of stimulation. Thus Lewin (Lewin, 1938) points out:

> "One reason for this roundabout route in scientific thinking is that rather small apparent differences can be representative of important dynamical differences (and vice versa) and that a change of dynamical facts in one direction does not necessarily lead to a change in the same direction in the resulting symptoms. For instance, a slight degree of anger might express itself openly with relative ease. However, an increase of intensity of anger usually leads, not to an increase of anger expressions but to a quieting down."

Intervening variables are not measured in the centimeter-gram-second system, although knowledge about them does flow from such measurements. Hull, for example, makes a first approximation to a definition of his unit of habit strength (the hab) (Hull, 1943) by considering the increase in habit strength associated with an increase in the number of reinforcements, under the circumstance that the two quantities are related by a simple growth function. Growth functions have the property that the rate of increase in the dependent variable is proportional to the magnitude of the other variable. Hull uses a growth function in which the proportionality is such that the larger the number of reinforcements, the smaller the increment in habit strength which is due to an additional reinforcement. Anyone who studies factors associated with habit strength, therefore, must base his conclusions upon observations on the number of reinforcements, or some other correlated dependent variable or combination of dependent variables, which are accessible to observation either as measurements or counts.

Some theorists use intervening variables as if they denote states in the nervous system, which in the present state of our knowledge, cannot be investigated directly. Hull believes that the habit whose strength is measured by the hab exists as an invisible condition of the nervous system. Spearman was somewhat less confident of the physiological status of his "g," but conjectured that intelligence (denoted by his "g") was a central limited quantity of nervous energy which could be diverted into various specific channels. Pavlov, on the other hand, was confident that his conceptualized inhibition found its proper locus in the nervous system itself. Other theorists who use intervening variables either have little interest in such denotations or believe that they are meaningless. Tolman, for example, states that our foremost effort as psychologists must be devoted to the collecting and ordering of molar behavior facts as such, a task which can be performed in relative ignorance of both physiology and neurology (Tolman, 1932). Lewin in his discussion of psychological forces sees no need to raise any question as to the physiological basis of his forces. They are justified, he states (Lewin, 1938), if the logical-mathematical properties of the intervening variable are clear, and if means exist by which one can determine

in a concrete case, whether or not they exist, i.e., if they are operationally defined.

Intervening Variables May Be Used as Independent Variables.—Once Lewin's two criteria are met, no barrier exists which prevents the formulation of investigations in which definable aspects of behavior are investigated as functions of a given intervening variable. In such experiments the *independent* variable is an intervening variable. Such investigations are found among the reductionists as well as the nonreductionists. Hull, for example, has been interested in the factors which are responsible for the production of reaction potential (Hull, 1943), an intervening variable which denotes a "reaction-tendency." Among these factors (these are the independent variables) he chooses habit strength (as reflected by number of reinforcements used to establish the habit) and strength of drive (as reflected by the length of time during which the organism has been denied a relevant satisfaction, such as food or sex. Since the dependent variable (reaction potential) is also an intervening variable, it appears that in such an investigation, all variables are intervening variables.

As an illustration of this type of experiment as conceived and executed by a nonreductionist, we may take certain aspects of Karsten's work. Among other things, Karsten studied the extent to which the subject would leave the field (i.e., abandon the goal set for him) and would satiate with respect to other tasks, as a function of the degree of his satiation with a certain task (Karsten, 1927). Satiation is defined by such criteria as variation in quantity of work, forgetting, lapses of attention, and loss of capacity to visualize the task as a whole. In this investigation, satiation of the central task is the independent variable, and abandonment of the task, satiation in respect of certain other tasks, and satiation in respect of all other tasks are dependent variables.

On the Formulation of Hypotheses.—Once concepts are defined and means have been devised which permits their use in investigative work, scientists use these concepts in the formulation of questions about nature. The kind of questions asked always reflect

the scientist's view on the fundamental issues with which we have been concerned. Psychologists who are materialistic monists, like Watson and many of the early behaviorists, will ask one type of question, while psychologists who are functional monists, like Tolman and many of the later behaviorists, will ask a different type of question. And the same is true of the psychologists who are reductionists as compared with those who are nonreductionists.

Within the universe in which they choose to work, however, *all* are subject to the same requirements as is the child who is faced with a novel problem. Each is required to perceive the existence of a problem, to formulate it, and to solve it. The study of this aspect of theory development is, in all its essentials, the domain of the psychology of problem solving. Hence the reader is referred to Chapter 10, which is concerned with an account of this special psychological problem, for an extended account of the relevant contemporary theory. Here we will merely note certain summary conclusions.

First, a distinction must be made between theories which presuppose inductive processes and theories which presuppose deductive processes. Inductive reasoning requires movement from the particular to the general. Deductive reasoning requires movement from the general to the particular. Inductive reasoning in psychology is illustrated by Weber's discovery of the fact that, within limits, the ratio of that increment of intensity to an existing intensity which produces a just noticeably different experience is constant. The Weber-Fechner law which states that the intensity of an experience is a logarithmic function of the energy of stimulation is a deduction from Weber's law. A contemporary example of inductive reasoning is the Helson law of the visual adaptation-level, which states that such an adaptation-level is a certain function of intensities from the figure and ground of a visual field (Helson, 1948). From this law verifiable deductions can be made not only as to color constancy and color contrast but also in other areas of perception and conception. Deductive theories make heavy demands upon formal logic since, by nature, they consist in the establishment of consequences which follow from stated premises. Inductive theories make

less demand upon formal logic, and greater demand upon the capacity of the theorist to have insight.

Second, while it is true that many theories are monuments to remarkable insights, others have been developed by pedestrian methods, including guessing and reasoning by analogy. The former type of theory rarely survives, but theories which have had their origin in analogical reasoning have been common, useful, and sometimes long-lived. Among such theories, for example, are those which dealt with perception after the analogy of chemistry. Such theories formulated the problem of perception as requiring the enumeration of the mental elements, and the discovery of the laws of their combination. Despite the fact that they are rarely used today, the elementaristic theories of perception are responsible for much of our present knowledge of sensory processes. Similar theories are to be seen in the history of action, and indeed are current today, in the work of psychologists like Ellson who is attempting to formulate theories of action which are suggested by the theories which have arisen in contemporary research on communication circuits (Ellson, 1949).

Third, as Wertheimer (1945) has shown, an important impediment to the development of a clear theoretical conception is the limiting effect of past experience, in particular as that past experience is represented in existing postulates, and in habitual methods of using familiar concepts. Theories that deal with difficult problems, as Wertheimer points out in the case of Einstein's work, must often be formulated by setting one's thinking in opposition to a very strong gestalt—in Einstein's case the structure of Newtonian physics.

Fourth, insight sometimes occurs at the level of antecedents and in other cases at the level of consequences; and theories are sometimes formulated by considering the consequences of conditions, and at other times by considering the possible antecedents of an observed fact. In either case the psychological process must be distinguished from the logical construction which is used to communicate the theory and its proof to the scientific public. The psychological analysis of problem solving makes heavy demands upon

the concept of the structure of thought, the transposability of structure, the concept of the good gestalt, and upon the making of distinctions between forces which are characteristic of the thinker himself (his tenacity, knowledge, etc.) and of the properties of his conception of the problem. According to such a view, any problem properly conceived will solve itself, and indeed, such often seems to be the case when we are dealing with "easy" problems.

Summary of Our Examination of the Nature of Concepts.—The previous sections have enumerated three distinctions which can be made among the kinds of concepts used by scientists generally, and psychologists in particular, in developing their theories. Distinctions can be drawn between the concepts of the reductionists and the nonreductionists, between concepts operationally defined and nonoperationally defined, and between concepts which refer to quantities or qualities which are subject to direct examination, as against those which can be studied only indirectly. No serious difference of opinion exists among psychologists today as to the necessity for the operational definition of concepts, although there are some who dissent from the statement that operational definition is both necessary and *sufficient*.

There is also little disagreement as to the validity of the intervening variable. Both reductionists and nonreductionists use intervening variables, and the present literature of experimental psychology having to do with their study is equally large, no matter whether we examine the nonreductionist literature or the reductionist literature.

The distinction between reductionist and nonreductionist concepts is the locus of some of the most important differences of opinion in contemporary psychology. Schools of thought which are wholly identified with the objectives of reductionism are sometimes apt to regard the work of nonreductionists as being unscientific in an essential sense. Differences of opinion as to whether a theory is or is not a reductionist theory are also to be seen. For example, Lashley and Wade (1946) criticize Hull's theory of stimulus generalization, in part, on the grounds that it is merely a restatement of the law of association by similarity. In the view of the critics,

Hull's theory is coordinate with another psychological theory, rather than superordinate to and dependent upon known physiological fact and theory. In a general comment upon present-day theory of perceptual similarity, these authors note that both associationistic and holistic systems tend to rely upon a conception of a gradient of relations as forming the basis of perceptual similarity Such a gradient, however, has no substantial relationship to anything known about the nervous system or about the mechanism of the transmission of nervous excitation On that account, the discovery of the basis of similarity is a problem whose solution depends upon the discovery of principles of nervous integration as yet unknown Since similarity cannot be reduced, neither can stimulus generalization, and in the view of the critics, Hull's theory is not truly reductionistic [2]

The Essential Unity of Psychology. –The divergence in opinion which has been described in the preceding paragraph illustrates one of several sources of divergent opinion, which the sophisticated student cannot fail to observe These differences are often so challenging of the student's attention that they divert him from observing those similarities in the work of representative psychologists which are more than strong enough to bind them into a single scientific tradition Two of these similarities are of particular importance Irrespective of the difference in their views, Lashley and Hull are in agreement on the fact that psychologists, like all scientists, are engaged in the work of studying the interrelations among concepts, in particular, their functional interrelationships Secondly, they are in agreement that the work of studying such functional relationships suffers in the extent to which it fails to meet the requirements of the experimental method All psychologists are united in the study of behavior by means of the investigation of the functional dependence of concepts, through the use of the experimental method.

Experimental Method

What Is an Experimental Method? Theories which with-

[2] For his reply to this criticism see Hull, 1947

stand the logical analysis of their concepts are tested by determining facts, and great support is always brought to a theory when it can be stated that the facts were determined by the experimental method. On this account it is a matter of importance to be able to distinguish between a nonexperimental procedure and an experimental procedure. It is evident immediately that any question as to whether the procedures are experimental or nonexperimental is unaffected by the merit of the hypothesis in question. Two experiments may appear to differ greatly in value because they test hypotheses which have different degrees of merit, one of them affording great insight into the organization of nature and the other affording little or no such insight. Despite this difference, the experiments may be equally well designed, and equally capable of determining the facts at issue: from the formal point of view no difference exists.

It is also evident immediately that if we are dealing with the methods by which knowledge is developed, then we cannot distinguish between experimental and nonexperimental science on the basis of content. The old-fashioned practice of defining *experimental psychology* as that part of psychology which had to do with perception, memory, thinking, and action is generally recognized today as being of historic significance only, having no bearing upon the question with which we are concerned.

Efforts have been made to define the experimental method on the basis of the fact that it necessarily involves the direct manipulation of the conditions affecting the phenomena under investigation, in particular the independent variables. Such a definition leads to the distinction between experimental and statistical investigations, the former being regarded by many psychologists as being more dependable than the latter. If such a defining criterion is taken seriously it leads to difficulties and fails to account for crucial similarities. Astronomers, for example, are usually regarded as experimental scientists. Yet the astronomer works without any control over his independent variables. It is true that the astronomer controls the apparatus and the surrounding conditions under which he uses his apparatus during the course of his work, but in no sense of the word does he control the phenomena toward which his work is oriented.

Despite this fact, when he reports the perturbations in the orbit of a planet as a function of the position of another planet, as a result of observations which he has made, such work is usually credited to the experimental method.

As we have noticed, the astronomer is not without control over the conditions under which he makes his observations. This control may be one of two kinds. He may control certain of his conditions in the sense that he can pre-establish them He can pre-establish the position of his telescope and the rate at which it moves during the interval in which he takes his photograph He can pre-establish the type of negative on which he takes his photograph He pre-establishes the properties of the mirror which forms the essential element of his telescope by specifying the properties to the lens-grinder. He may also control the conditions under which he makes his observations in the sense that he causes them to be measured, and records the results of the measurements as they occur. Thus the temperature of the mirror may be recorded during the course of his observations, as well as the functioning of the power drive by which the telescope is maintained in the pre-established position.

While these two kinds of control are found to have their counterparts in experiments throughout the whole range of science, it is still a fact that the control of the conditions only partly constitutes the defining property of the experimental method, since experiments exist in which an important property is intentional *absence* of control. Such experiments are very common in agriculture and in genetics, and they are becoming increasingly common in psychology. In such experiments, the objective is to see whether a difference in productivity, or in some other dependent variable, due to a treatment, is sufficiently large to exceed productivity differences which occur whenever no control is exerted. Thus, two groups of subjects may be shown to differ in the amount of their recollection, accordingly as they have different attitudes, to an extent which exceeds the differences among those who exhibit the same attitude (Levine and Murphy, 1943) and such an investigation is commonly called an experimental investigation.

Knowledge of the Reliability and Validity of the Procedures Defines an Experimental Method.—One of the most common of the properties which attach to the results of experimental work is the confidence with which they are offered and accepted. If the latter type of experiment is compared with the classical type in which direct control is exerted over the independent variable or variables, it is evident that both contain elements of procedure which are designed to increase such confidence. This confidence does not have the *a priori* basis of the confidence which attaches to the results of mathematical work. It has its basis rather, *a posteriori,* in evidence as to the consistency with which results, as reported, will be encountered if the work is repeated under the same conditions, and in evidence that the independent variables have been correctly identified To illustrate this latter distinction: if you are informed that an experiment shows that the apparent size of a circular disc is a function of the degree of illumination in the visual field, you have a right to expect that the assertion can be supported by records of a number of tests of the hypothesis, as well as records that report procedures which exclude all possibilities except brightness of the field as the factor which produces the change in the apparent size of the disc. Or, if you are told that uneducated people are more likely to deny the aspirations of a minority group than are educated people, you should expect to be shown that the results would be unchanged materially if a number of tests were made of this fact, and that the possibility of the difference being due to disparities in economic status or age or some other factor, has been excluded by some feature of the procedures by means of which the fact was established

The distinctions which have just been made are very similar to distinctions which are made in modern psychometrics. If the results of the application of a psychological test are to be taken seriously, the test must be shown to have met two requirements. In the first place it must yield substantially the same return from two applications under the same conditions. In the second place, it must afford a basis for a reasonably accurate prediction of the trait which it purports to measure. The first of these requirements is known as

the requirement of reliability, and the second as the requirement of validity.

An important property of the concepts of reliability and validity, as they find application in test construction, is the fact that both possess a quantitative basis. Test reliability is measured by the coefficient of correlation calculated from two applications of the test. Test validity is measured by the coefficient of correlation obtained from test scores and from independent measures of the trait which the test purports to measure.

Like test reliability, experimental reliability has a quantitative basis. We say that an experiment is reliable when it produces evidence not only as to the fact, but also as to the numerical range through which the observations on the fact may be expected to be found in additional experimentation. The statement that 40 per cent of an uneducated group deny the aspirations of a minority group, while only 20 per cent of an educated group deny these aspirations, cannot be taken seriously unless the procedures by means of which the fact was determined are such as to permit an additional statement. We need to know the range through which such percentages will vary if the observations are repeated We need such information in order to form a conclusion as to the possibility of a contrary result being observed.

Statistical methods are often thought of as having to do with techniques for the classification and description of data. Of greater importance is the function they perform by which from a consideration of the internal structure of the results of an experiment, one can form a conclusion as to whether the results would be substantially duplicated if the experiment were repeated under the same conditions.

The experimental method is incapable of producing an absolute result. Any fact, whether it be a measurement or a count, reported from an experiment, is always subject to a standard error, i.e., a number which defines the range through which the measure can be expected to vary in subsequent experimentation. The presence of the ubiquitous standard error is due to the fact that no sample can be completely representative of the population from which it is

drawn, or that no measurement can be made which does not require some degree of estimation in the final decimal place. In the crucial case, no experiment would be taken seriously, if it were reported that the experimental determination has no standard error.

It is evident that no experiment can yield a *certain* conclusion as to a question of fact. It is also evident that experiments yield information on the basis of which we can graduate our confidence as to the stability of a conclusion. All procedures which are called experimental have these properties in common. Hence they can be called defining characteristics of the experimental method.

A second class of defining properties of the experimental method are associated with the provisions which are made for the determination of the validity of the experiment. Experimental validity has to do with the identification and evaluation of the dependent and independent variables. The history of experimental psychology abounds with instances of experiments which have required very extensive collateral investigation to justify the validity of the conclusions. For example, recent years have seen the development of an extensive literature on the identification of the patterns of nervous impulses in the auditory nerve, and their correlation with the properties of the energy furnishing the stimulus for hearing. This work was very carefully carried out in its early stages (Wever and Bray, 1930) in order to make sure that the functional relationship of the stimulus and the response was not a result of such an artifact as would be present if some unknown connection existed between the stimulating apparatus and the detecting and recording apparatus. In order to exclude this possibility, Weaver and Bray demonstrated that the reproduction of the tones used in stimulating their preparations occurred only when the preparations were alive.

A second illustration of this point can be offered from the field of social psychology. Bavelas reports that if a group of dispirited leaders who have been using relatively authoritarian methods in their direction of the activity of children are trained in a certain way to use democratic procedures, one will observe an increase in the effectiveness of the leadership accompanied by independent evidences

of an improvement in the morale of the leader (Bavelas, 1942). This conclusion can easily be used to support the more general conclusion that authoritarian leadership is inferior to democratic leadership. In order to perceive the limitation which exists upon such a conclusion, one must know that the authoritarian practices characteristic of these leaders were largely self-taught. It is clear, therefore, that the differences between the leaders, before and after training, are of two kinds. In the first place, we can distinguish between self-taught leaders and leaders who have been taught by an expert. In the second place, we can distinguish between the leaders in the authoritarian context and in the democratic context The experiment has not excluded the possibility that the differences in efficiency and morale are due to the differences between lack of training and effective training, rather than to the difference between authoritarian and democratic techniques

Our two illustrations indicate the ever-present possibility that a dependent variable may be a function of an unrecognized independent variable rather than the stated independent variable Valid experiments include elements in their design which afford a basis for judgment as to whether the results are a function of the stated independent variable.

On Constant and Random Errors.—The modern theory of experimental reliability and validity leans heavily upon Gauss' theory of the errors of measurement (Coolidge, 1925). It is known that whenever successive determinations of a fixed magnitude are made, identical outcomes are rarely observed The departures from the true magnitude are of two kinds Those which occur with the same algebraic sign and which act to bias the measurement in a single direction are known as constant errors Those which occur with positive and negative signs, which increase the experimental value over the true value in some cases and decrease it over the experimental value in other cases, are known as random errors. The average of a set of constant errors is usually some value other than zero, while the average of a set of random errors is usually zero or some quantity which differs from zero by a negligible amount On the assumption that large random errors are less likely than

small random errors, Gauss was able to show that such errors distribute according to the normal law. Once this fact had been demonstrated it was possible to associate known probabilities with random errors of any magnitude, a fact which lies at the basis of all statistical theory as it relates to the reliability of observation.

Although the Gaussian theory of the distribution of random errors was developed originally to account for random fluctuation of measurements, it is equally applicable to problems involving the random fluctuation of counts. Thus known probabilities can be associated with random deviations in a frequency or a percentage, a fact which is indispensable to the experimental study of behavior like attitudes and opinion in which responses must be classified rather than measured

Factors Determining the Reliability of Experiments.—An experiment gives reliable results if we have reason to believe that the random errors are small, i e, that repetition of the experiment will reveal little variability in the outcomes Since the outcomes of an experiment are generally expressed as either measurements (or statistics based on these measurements like the mean or the median), or counts (or statistics based on the counts like percentages), reliability is evaluated by measures of the consistency of such statistics when the procedures are repeated There are two ways of evaluating such consistency. The most persuasive evaluation is a statement of the variability observed when the procedures are in fact repeated. Nothing can compete with such a measure in establishing the reliability of an experiment In the absence of one or more repetitions of the entire experiment, however, reliability can be estimated from a knowledge of the internal structure of the results, provided certain necessary conditions are fulfilled

If neither of the foregoing two methods can be used to estimate the reliability of the outcomes of a procedure, we have no grounds for accepting it as an experimental procedure If, on the other hand, reliability can be assessed, and it is discovered that it is low, then the procedure can be described as an experiment which possesses relatively little precision. Whether the experiment is unsuccessful as well depends upon other things; experiments which possess rela-

tively little precision may still possess enough to settle the question to which they are directed. However, experimental scientists usually want all the precision obtainable; hence they must not only have access to devices for evaluating precision, but also to devices by means of which it can be increased.

Precision is always affected by the number of observations made in any one series, and by the variability in the individual observations over the series. In psychological investigations the number of observations may be increased in one or both of two ways. The number of subjects may be chosen at the experimenter's will, as may the number of observations per subject. Wherever the outcomes are taken over a series of subjects, the larger the number of subjects, the smaller will be the variability in any measure of statistical reliability which relates to the series The same principle holds wherever a number of observations are made on the same subject In addition, other things being equal, the less the variability in the observations made over a single series of subjects or over a single series of observations on the same subject, the less will be the variability in any measure of statistical reliability which relates to the series It follows that experimental procedures are always cast in such a way as to yield information on the number of observations and their variability.

The experimental scientist can work with mere knowledge of the extent of his precision but he is never completely satisfied if he is restricted to such mere knowledge He always requires freedom to increase the precision if necessary Since the precision is conditioned by the ratio of a function of the variability of the observations to a function of the number of observations, it is clear that he can choose to increase the precision by increasing the number of the observations, or by taking steps to reduce the variability of the observations, or by doing both. While the former step can be taken routinely, the latter step always presents special problems; to reduce the variability of the observations, measures must be taken to discover and to eliminate the factors responsible for the variability.

In psychological investigations, random errors may be present because of coarse graduation of the stimulation or coarse gradua-

tion of the measurement of the response, because of lack of purity in the intervening processes, or because of the inherent variability which exists among subjects. To eliminate such factors, steps must be taken to increase the fineness with which stimulus and response are consistently measured or classified, or to purify the intervening processes, or to reduce subject-differences. Increasing the fineness with which stimulus and response are graduated may imply the use of physical instruments which are capable of detecting finer differences, but it can also imply the use of psychological techniques which fulfill the same requirement. For example, to investigate the relative transfer values of Latin to French and Italian, very precise techniques would be required for evaluating the various proficiencies, since the differences in the transfer values of Latin to French and Italian may possibly be slight.

Purification of the intervening processes often depends upon the clarification of the instructions given the subject in the experiment, and the consequent standardization of his set and attitude. Such purification is particularly necessary whenever more than one subject is used, since different subjects will interpret ambigious instructions in different ways. Purification is also necessary in experiments in which many responses are taken from the same subject, since the precise interpretation put upon an ambiguity may vary from trial to trial in the same subject. For example, in experiments in which a subject is required to report lesser, equal, or greater in comparing intensities which differ by no more than 4 to 16 per cent of the standard, the meaning denoted by *equal* may vary from comparison to comparison (Fernberger, 1930). Sometimes equal may be interpreted to mean *unequivocally equal*, other times it may mean *not clearly different*, and at other times *not greater and not lesser*. The precision of such experiments is a function of the clarity with which the *equal* category is defined.

In recent years experimental psychology has profited with other sciences in which precision tends to be low, by the development of a method for the planning and treatment of observations, known as the "analysis of variance." The analysis of variance performs two functions. It permits the study of the single and combined effects

of several independent variables upon a given dependent variable. Thus, in a single experimental procedure, the investigator is able to study the single and combined effects of color of the illuminant, intensity of the illuminant, and brightness of the background upon the visual adaptation-level (Helson, 1942). This property of the analysis of variance is having a powerful influence upon psychological theory, since it encourages theorists to deal with dependent variables as possible functions of several independent variables. The analysis of variance performs its second function in providing devices by means of which the variability attributable to various sources may be segregated and evaluated. This function has led to several valuable consequences, of which the stimulation to the experimental scientist to consider the benefits to be reaped from experimental designs which will keep error to a minimum is not the least important. Very important is the fact that the analysis of variance, when used with the analysis of covariance, is capable of *eliminating* a part of the random variation from the results, even though it has occurred during the course of the experiment. When the presuppositions of these techniques are realized, precision can be obtained which is comparable with what would be obtained if the elements of random error responsible for lack of precision were eliminated by use of one or more of the methods enumerated above.

The analysis of variance often leads to a comparison of the variability observed over a number of subjects with the variability observed within a single subject. Such comparisons invariably lead to the same conclusion; the variability from subject to subject in random samples is generally in excess of that variability which is seen in observations made over the same subject. This fact means that desirable increases in precision can be anticipated, if experiments which have been shown to lack precision are redesigned to permit multiple observations on the same subject

The specifications which are drawn for the conduct of an experiment constitute the experimental design. The most common form of experimental design is one which includes provision for a subexperiment by means of which the dependent variable can be studied in the absence of that treatment which is required by the theory.

Such sub-experiments are called control experiments. To discern the effect of adrenalin in the production of emotional responses (Landis and Hunt, 1932) it is common to inject two series of subjects, one with adrenalin and one with a neutral substance like saline solution, and then to compare the two kinds of subject on the basis of their response to relevant instructions. The series which receives the saline solution is the control series. If a sufficient number of observations are made, and if each subject is unaware of the identity of the substance used on him, the design is adequate for the determination of the reliability of the method, and also provides for its validity.

Factors Determining the Validity of Experiments.—The validity of an experiment cannot be evaluated without an intimate knowledge of the theory, since the validity is assessed by knowledge of the extent to which the outcome can be regarded as a *specified* function of the stated independent variables. Some theories depend for their substantiation upon observed magnitudes in the dependent variable, others depend upon observed differences between values of the dependent variable for each of two values of the independent variable, while others depend upon the algebraic sign of such differences. A special case of the latter is the case in which the theory predicts an order of merit in the magnitudes of a set of dependent variables. Whether or not an experiment is invalid in a given case depends upon whether magnitudes, differences in magnitudes, or the algebraic signs of differences in magnitude is specified by the theory. If absolute magnitudes are specified, then the experiment lacks validity when constant errors are present in the measurement of the dependent variable The same principle holds if the dependent variable is evaluated by frequencies or percentages rather than measurements. If the size of a difference is specified by the theory, the presence of the same constant error in the results from the application of two or more values of the independent variable will not disturb the validity of the experiment. The same conclusion holds if the theory specifies only that the outcomes will be observed to conform to a defined order; the presence of a uniform constant error in the

dependent variable at all levels of the independent variable or variables will not invalidate the experiment.

Constant Errors as Related to the Validity of Experiments.—Specific theories in psychology rarely demand absolute values for their testing; they are most likely to demand knowledge of the direction of a difference, and somewhat less likely to demand knowledge of the amount of a difference. On this account, investigators are often more intent upon keeping conditions constant over the series of independent variables than upon identifying conditions So long as the conditions are kept constant, the investigator knows that his investigation will be capable of yielding knowledge at the level required by his theory, if that theory predicts only the sign or magnitude of the difference

It is not unusual for constant errors to be constant only for a restricted set of conditions, for a different set of conditions they take other values. In reaction time experiments, the use of a single chronoscope will occasion no difficulty in drawing conclusions as to the difference between the reaction-times associated with different kinds of stimulation, since any constant error in the measurement of the times, due to a constant idiosyncrasy of the chronoscope, will be the same for all measurements If two chronoscopes are used, each having its own constant error, difficulty will arise unless an equal number of observations is made with each on each species of stimulation. If, in an experiment to compare visual and auditory reaction times, all visual trials are made with the one instrument, and all auditory trials with the other, the visual reaction times will contain one increment due to the constant error of its chronoscope, while the auditory times will contain another increment due to the constant error of the other chronoscope For that reason, the investigator will not be able to state whether any difference or lack of difference in the times is due to the difference in the conditions of stimulation or to the difference in the chronoscopes

Constant errors are capable of very subtle behavior. The extent to which a subject develops a conditioned response is a function of the attitude of the experimenter. This attitude acts to produce constant errors in the records of the conditioning. The attitude of the

experimenter undergoes subtle changes as he progresses in skill. The change in the attitude acts to produce changes in the constant errors in the records of the conditioning process. Hence it becomes difficult to conclude whether differences in rate of conditioning are due to differences in the experimenter's skill or to differences in the independent variable of the experiment. In similar vein, changes in the subject associated with fatigue, learning, the acquisition of expectations, changes in interest and rapport, can act to produce a change in the constant errors of the experiment which increases the difficulty in drawing unambiguous conclusions from the results.

When variation in the dependent variable due to variation in a constant error becomes so great as to mask out the effects of the independent variable, the experiment becomes completely invalid as a test of the hypothesis. Where the investigator is aware of the circumstances no harm is done, since he can repeat the procedures with suitable modifications. Where he is unaware of the effect, he is bound to deal with his theory on a mistaken basis.

Lack of validity may be occasioned by inadequate knowledge at the level of the conditions of stimulation, at the level of the intervening processes, at the level of the registration of the response, or at the level of the selection of the subjects. In the case of reliability, concern is felt for the extent of random deviations in each of these four respects. In the case of validity, concern is felt for the extent of uni-directional deviations, particularly when they are correlated with the treatment represented by the independent variables of the investigation. In all cases, to establish the validity of an experiment, accessory knowledge is required. In the case of invalidity attributable to knowledge of the conditions of stimulation, knowledge that changes in the dependent variable are due only to change in the stated independent variable is required if any challenge is to be answered satisfactorily. Where electric shock is used as a reinforcement agent in learning experiments, for example, it has to be shown that it is only the electric shock, and not the shock plus some masking agency in the cutaneous tissues which acts to vary the effective shock in accordance with some law (Jenkins, Warner and Warden, 1926), which is the independent variable in the experiment. In the case of invalidity attributable to the level of the intervening process, it must

be demonstrated that changes in the dependent variable are not due to changes in such conditions as fatigue, interest, definition of terms, suggestibility, or attitudes. In the case of invalidity attributable to the conditions of the response and its registration, knowledge is needed as to the extent to which the making and registration of the response contribute in some unidirectional manner to the value of the dependent variable. In the recording of muscular action this may require a special study of the extent of the arcing error (Davis, 1948; Wendt, 1938); in the study of attitudes it may require a special study to determine the optimum length of a questionnaire.

Lack of validity due to the selection of subjects is of peculiar interest in psychology since many experiments rest upon the comparison of the behavior of two groups of subjects, each group having had separate treatments Such experiments are valid only when both groups of subjects are composed in such a way as to justify the beliefs that they are equal in all respects save the treatment This belief is justified in one of two ways. The subjects may be studied in specific respects prior to the experiment and be shown by the preliminary study to be equal In such a case the groups are said to be equated in known respects On the other hand, they may be drawn at random from a defined population, with reliance being placed upon the theory which states that if samples are drawn randomly from the same population the larger the samples the less likely that they will differ materially in any given respect

The drawing of a *random* sample is a task requiring special knowledge A sample is said to be drawn randomly when each member of the population has an equal chance of being drawn, and when the drawing of any one member has no effect upon the likelihood of any other member being drawn To realize this condition it is necessary that, in fact, all members of the population be accessible to the drawing and have their equal chance of being drawn. Special techniques are required for the making of the drawing, once the population is made accessible

Whenever a sample purporting to be random is, in fact, unrepresentative of its population it is said to be biased. An error of bias occurs whenever a statistic based upon a sample differs significantly from the *comparable* statistic, usually known as a *parameter*, based

upon the population. It is known that the grades of students are related to the seats which they occupy in the classroom, those sitting closer to the instructor (where choice of seats exists) receiving somewhat higher grades. It is very easy on this account to introduce a serious bias into a learning experiment by composing two groups of subjects, one from the front row of a class and the other from the rear row of the same class. Such a design would confront the experimenter with the problem of distinguishing the effects of the treatment from the effects of likely pre-existing differences in ability. If the samples to be drawn are large, the instructor may proceed by drawing two random samples, in which case, each will be representative of the same population and he need not fear the intrusion of an error of bias. Where the numbers to be drawn are small, it is much better to establish the groups by equating.

The problem of bias is particularly troubling in the testing of theories which have to do with group membership, since such theories demand experiments in which the samples studied can be shown to be representative of their respective groups. When such groups are as numerous as that constituted by the totality of ten-year-old boys, or American adolescents, or the adult human male, or the American Negro, the problem becomes almost unsolvable within the limits of the resources available for research by social scientists. This fact is reflected in the reserve with which psychologists and other social scientists deal with tests of theories which relate to group membership as well as the suspicions which often attach to experiments which use statistical methods for graduation of independent variables.

Two Risks Are Taken in the Use of Experimental Data.— Experimental data are taken to test hypotheses or theories. After the data have been taken, the experimenter faces the problem of using the data as a basis for the formation of a conclusion as to the soundness of his theory. Where the results are clear-cut, the conclusion forms without his being conscious of the presence of any risk that he may be mistaken. Where the results are less than clear-cut, consciousness of risk is increased, and may be so great as to induce him to suspend judgment until further evidence can be assembled. De-

spite the lack of feeling of risk which accompanies the presence of clear-cut results, it is still true that risks are being run. These risks arise from two sources. The reliability of his experiment is not absolute--the experimenter is bound by the fact that his results could occur in random sampling, no matter how small the percentage of trials. In addition he is at the mercy of unknown factors which may have affected the validity of his experiment.

Two kinds of risk taken by experimental scientists have been intensively studied by Neyman and E. Pearson. In defining these risks, Neyman and Pearson (Neyman and Pearson, 1932, 1936) distinguish between experimental hypotheses and statistical hypotheses. The experimental hypothesis is a statement of the expectation as to the way in which the dependent variable is functionally related to the independent variable or variables. Experimental hypotheses are usually tested by supposing that no dependence exists and by calculating the likelihood of the observed result on the assumption that it is wholly due to chance. The process of calculating the likelihood that an observed outcome would occur in random sampling is referred to as *testing the null* hypothesis. The null hypothesis is a statistical hypothesis. If the null hypothesis is accepted, the experimenter is bound to regard his experimental outcomes as being wholly due to chance. If the null hypothesis is rejected, the experimenter is then at liberty to conclude that a certain type of functional dependence exists among his dependent and independent variables, provided of course that he can defend the validity of his experiment.

The two kinds of risk studied by Neyman and Pearson are: (1) the risk that the experimenter will *reject the null hypothesis* when it is really true, and (2) the risk that the experimenter will *accept the null hypothesis* when it is really false. The former risk leads to Type I errors. Such errors occur when the experimental hypothesis is accepted when it should be rejected. If we are testing the hypothesis that auditory reaction times are longer than visual reaction times (Woodworth, 1938), an hypothesis which should be rejected in the light of all modern knowledge, a Type I error would be committed if the hypothesis were accepted. If we are testing the hypothesis that visual sensitivity is dependent upon amount of pig-

mentation in the eye (Helson and Guilford, 1933), an hypothesis which should be accepted in the light of present knowledge, a Type II error would be committed if the hypothesis were rejected

Type I and Type II risks are implicit in all experimental work, but they have become especially explicit in certain kinds of experiments peculiar to psychology In the development of instruments for the prediction of behavior, for example, a personal adjustment inventory for the diagnosis of hypochondriasis, attention must be directed to two kinds of failure which such instruments may reveal The test may fail by classifying a subject as an hypochondriac when he is not an hypochondriac It can also fail by classifying a subject as *not* an hypochondriac when in fact he is Subjects are said to be *false positives* or *false negatives* according to the type of error which has been made A false positive is a subject who is wrongly identified as exhibiting the trait and a false negative is a subject who is wrongly identified as *not* exhibiting the trait It can easily be seen that these two types of error, and hence the risks with which they are associated are interdependent One can adjust the critical score (the minimum score diagnostic of hypochondriasis) on the basis of which the diagnosis is made in order to decrease the number of false positives By doing so the number of false negatives is thereby increased Similarly any adjustment designed to decrease the number of false negatives will increase the number of false positives

Neyman and Pearson have shown that the same principles hold in the case of Type I and Type II risks generally If we adjust our definition of statistical reliability in order to reduce the chance of committing a Type I error, we will increase the chance of committing a Type II error Similarly if we act to reduce the likelihood of committing a Type II error we will increase the chance of committing a Type I error

Neyman and Pearson have also shown that the total likelihood of being wrong is a function of the kind of statistical procedures which are used in the making of the analyses of the reliability of the results of the experiment Such risks are known to be kept at a minimum whenever the design of the experiment is such that the experimental outcomes may be tested for their reliability by the

use of statistics which are based either on the normal curve, or upon a series of related curves, first used by William Sealy Gosset (known in the literature as Student) which describe distributions commonly called t-distributions.

The distinction between the Type I and Type II errors provides an additional insight, which can be illustrated by the test situation described earlier. In some practical situations one is concerned with minimizing the false positives. To do this the cutting score is set sufficiently high to eliminate any nonhypochondriacs who happen to have high test scores. In other practical situations we are concerned with eliminating false negatives and, in that case, the cutting score is set sufficiently low to include any hypochondriacs who happen to have low test scores. The experimental scientist is sometimes moved by practical considerations to follow a similar course. Where a theory has a far-reaching influence upon scientific theory he may be obliged to achieve far greater reliability in order to reduce the possibility of a decision which in the test situation would produce a false positive. This obligation is imposed upon him because of the unwillingness of his colleagues to accept a result which flies in the face of well-established theory. Where, on the other hand, no great violence is done to existing theory the obligation does not exist, and he will often establish his reliability in a way which equalizes the Type I and Type II risks.

The Neyman-Pearson theory of the risks associated with statistical tests has to do with the theory of statistical reliability, in particular with the capacity of specific tests of statistical reliability to support conclusions to which minimum risks attach. The theory presupposes that the statistical tests are conducted on data which have been obtained from valid experiments. It is possible, nevertheless, to extend the general tenor of their views to the problem of experimental validity. The factors which are responsible for lack of experimental validity result in the assumption by the experimenter of risks which are of the same nature as the Neyman-Pearson Type I and Type II risks. These risks unfortunately are not capable of the treatment which can be given to the Neyman-Pearson risks; they can only

be minimized by painstaking attention to the demands of the theory and to the conditions of the experiment. They are never completely evaluated, and on that account the experimental scientist is always affected by an uncertainty which is additional to that which is imposed upon him by the Neyman-Pearson risks.

The beginning student in psychology is apt to think of statistical methods as devices for the classification and description of data. The history of statistics since the time of Karl Pearson and Student has been a history of the coalescence of statistical concepts with the concepts of experimental methodology. Two important milestones in the history of that coalescence are the development of the Analysis of Variance by R. A. Fisher and his followers, and the statement of the theory of the power of statistical tests developed by Neyman and Egon Pearson.

On Reading the Experimental Literature.—As scientific sophistication increases, the need to be acquainted with original scientific sources of publication increases. Skill in the use of these sources is an asset to the student; like any skill, such a skill can be improved by intelligent direction.

Scientific papers usually give an account of the theory which led to the conception and execution of the experiment, an account of the procedures used in the investigation, including the sampling techniques, the treatment accorded the subjects, and the treatment accorded the results, as well as a discussion of the conclusions which have been formed on the basis of the results. Such papers do not ordinarily identify the dependent and independent variables as such Ordinarily they do not specify whether the concepts used are suitable to a reductionism or to a nonreductionism. It is fashionable in the literature to claim that the concepts satisfy the requirements of operationism, but rarely do authors raise any question as to the *sufficiency* of the operational definition of their concepts. And often no specific mention is made of the fact that the variables are variables suitable to direct observation or, contrariwise, are intervening variables. Conscientious experimenters report on the statistical reliability of their results, and on the measures taken to establish the validity

of their experiment. Such experimenters are explicit, also, as to the possibility of uncontrolled aspects of the conditions affecting their conclusions. Often they will go so far as to suggest hypotheses alternative to the hypothesis which they are willing to accept.

Where the experimenter is silent, he is presuming, in many cases on the reader's capacity to bring facts and attitudes to the reading which will help to make the report intelligible. If he is reporting an experiment which tests an aspect of reinforcement theory, he may assume that the reader has brought previous acquaintance with reinforcement theory and its contrast with gestalt theory to the reading of the report. He may also assume that the reader is familiar with the general principles of experimental design, and the statistical treatment of data. He no doubt supposes that the status of the concepts used by the reinforcement theorists is known to the reader.

Many of these assumptions are unwarranted in the case of the unsophisticated reader. For that reason, the reading of an original scientific paper by an unsophisticated person is often a confusing and unrewarding experience.

The first step to take to reduce confusion in the understanding of the content of an experimental report is the asking of the questions: "What are the dependent and independent variables of this experiment?" "What conditions are being specified by the experimenter?" "What quantity is being examined as a function of these specified conditions?"

Once the reader has answered these questions, he should ask himself about the status of the dependent and independent variables "In what units are they measured?" "Are either or both of them intervening variables?" "Is the discussion of the results concerned with intervening variables, and the experimental work concerned with variables which are capable of direct observation?" Answers to questions such as these throw light upon the nature of the theory which has led to the experiment.

Once the dependent and independent variables have been identified, the reader should ask himself, "What functional dependence of the variables in this experiment is *expected* by the experimenter?"

Often this question cannot be answered from the report, because the experimenter has done an exploratory experiment, the rationale of which includes no specific theory as to the relationship between his independent and dependent variables. Such a fact is not to be taken as a reflection of discredit upon the experimenter, it must, however, be recognized by the reader if he is to put the report into proper perspective. The reader who has discerned the experimenter's expectation as to the functional relationship of the dependent and independent variables should now ask himself, "What grounds exist for this expectation?" The answer to this question presupposes a grasp of the theory which lies behind the experiment. The complete answer may require the consulting of other sources, in particular those sources which conscientious authors give in the introduction to their paper

After the reader has identified the variables and has achieved a grasp of the basis for the experimenter's expectations as to their relationship, he is ready to deal with the report at the level of its experimental design "What results are reported?" "Are they accompanied by measures of their statistical reliability?" "Is the reliability sufficient to justify the acceptance of the hypothesis, or must it be rejected?" "Is the reliability such that additional evidence is needed before the decision to accept or reject the hypothesis is taken?" "What uncontrolled conditions exist in the experiment?" "Is there any likelihood that these conditions are responsible for the variation or lack of variation in the dependent variable?" "What other sources of variation besides those mentioned by the experimenter may account for the observed variation or lack of variation of the dependent variable?"

The foregoing questions are landmarks in the present chapter, and their answers are landmarks on the road to knowledge. In reading a report of an experimental investigation, and in raising such questions, the reader passes the same landmarks as did the theorist himself. When all are answered both have traveled the same road. Only after that road is traveled by persons other than the theorist do his assertions become incorporated into the grand body of scientific knowledge.

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CHAPTER 2

SOME PROBLEMS OF NERVOUS FUNCTION

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Psychologists and biologists alike, whose interest is in the behavior of man, now generally accept the fact that, as Adrian says, "the human mind is . . . anchored to the brain . . . The brain forms the sole connecting link between body and mind" (Adrian, 1947, pp. 6, 7). The psychologist and biologist both, however, no matter what their convictions as to the relationships of mind to matter, are yet unable to account for the production of mental phenomena by any mere physical knowledge they have of the brain proper. But as the biological sciences progress, the increasingly available information concerning the form and function of the central nervous system is enhancing the insight of the psychologist into some of his own special problems. For the benefit of the psychologists whose investigations impinge directly upon those of the physiologists, and for those who interest themselves in the central nervous system for the reason that the brain is the residence of mind, the following résumé is presented of some of the more compelling problems which confront modern investigators of central nervous function.

THE NEURONE AS THE STRUCTURAL AND FUNCTIONAL UNIT OF THE NERVOUS SYSTEM

The Establishment of the Neurone Doctrine.—Since modern investigators are prone to accept as axiomatic the statement that the neurone, which is a cell specialized for conduction, is the elementary structural and functional unit of the central nervous system, it is worthy of mention that the central nervous system was chronologically the last of the tissues or organs to be incorporated within the scope of the cell doctrine. Curiously and interestingly enough,

Schwann, who is especially familiar to neurologists by virtue of the neuronal sheath-cells which bear his name, and who was the first to establish the cell-theory for animal tissues, was himself mainly responsible for this paradox. He considered that the prominently visible sheath-cells lay down the nerve fiber, and hence that the nerve fiber is the production of a chain of cells rather than of a single one.

His theory, of great influence, was rivaled by another based on equally strong visual evidence, by which the fiber was assumed to differentiate *in situ* from protoplasmic bridges, under the influence of functional activity: those protoplasmic fibers in the embryo become nerve fibers which lie directly along the route of conduction. Both of these theories have subsequently, but only relatively lately, been supplanted by the ***outgrowth theory,*** first suggested by Bidder and Kupffer (1857), supported by His (1887) and by Ramón y Cajal (1890, 1892, 1894), and firmly established by the isolation of neuroblasts in tissue-culture and by other equally ingenious experiments of Harrison (1904, 1906, 1907, 1924).

According to this theory, the processes are outgrowths of the neuroblast itself, and a single self-contained cell is thus the primary unit of nervous tissue. This is now generally accepted by neurologists, although a few workers, some of whom claim to demonstrate intercellular connections at the synapse, others of whom interpret the myenteric plexuses as nonsynaptic nerve nets, tend to minimize the significance of such a discrete unit. This divergence of opinion now seems to the great majority of neurologists to be of little moment.

The classical outgrowth theory postulated that "the substance of the axone originates in the neuroblast and is spun out by means of the amoeboid activity of the end The outgrowth of the nerve fiber is, therefore, a mode of protoplasmic movement" (Harrison, 1935, p. 159). The most recent investigations, at this writing, suggest, on the basis of experiments which still require substantiation, that the growth of the fiber, "in the sense of production of new protoplasm, occurs solely at the base of the fiber in the nucleated part of the cell body" (Weiss and Hiscoe, 1948, p. 390), rather than

being caused by the amoeboid movement of the nerve end and the resultant spinning out of protoplasm behind it.

The analysis of factors controlling the direction of outgrowth of the fiber during ontogenesis may be of some interest, since it is related to some appealing but not necessarily valid theories which have solidly entrenched themselves into the literature.

Three kinds of factor have been postulated as influential in determining the direction of fiber outgrowth: chemical (first postulated by Ramón y Cajal, 1892), mechanical (first demonstrated by Harrison) and electrical. There has been no direct experimental

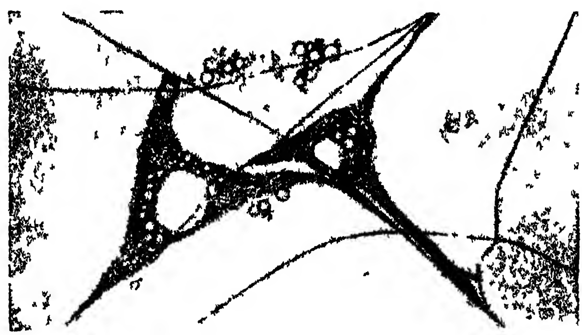


FIG. 21. Developing cells from medullary cord of frog embryo growing in serum and attached to spider web fibers. Magnification 500×. (From R. G. Harrison, *J. exp. Zool.* 1914, 17, 521–544, by permission of the author and the Wistar Institute of Anatomy and Biology.)

support for the theory of chemotropism. Harrison (1914) in some of his tissue-culture experiments demonstrated unequivocally, by means of spider-web inserted into the culture medium (Figure 21), that developing cells are stereotropic in their outgrowth, and this has been amply confirmed and extended in recent studies on developing neuroblasts (Weiss, 1934, 1941).

In part because of the great progress of electrophysiology and the successful applications of its methods to the study of the nervous system, there has long been enthusiastic interest in the possibility that electrical forces play their part in determining the direction of fiber outgrowth. This interest has been encouraged by the promulgation of Ariens Kappers' (1907) theory of *neurobiotaxis*, which on the basis of comparative studies on the topographical position

of particular cell groups in the vertebrate central nervous system postulates a shifting of centers toward correlated centers in a stimulopetal direction, under the control of bioelectrical factors resulting from conduction of the impulse. Ariens Kappers received strong support from Bok's (1915) theory of *stimulogenous fibrillation*, compatible with and allied to his own, based on the morphological arrangement of certain developing tracts in the embryo There is no adequate evidence to support the electrical aspects of either of these theories, although the topographical arrangements pointed out by Ariens Kappers and Bok are actual facts still remaining to be explained.

Ingvar (1920), some years ago, claimed that when weak currents (current density 0 001 to 0 002 μamps/mm.²) were passed through a tissue-culture containing developing neuroblasts, the outgrowing nerve processes followed the lines of galvanic force. These results have not been substantiated, and Weiss (1934) was able to reproduce them by inserting mechanically similar nonconducting elements in place of the electrodes, and to explain Ingvar's results on the basis of mechanical rather than electrical influences More recently, Marsh and Beams (1946), using current densities far greater than those of Ingvar, have again claimed positive electrical effects With the passage through the cultures of currents of densities greater than 120 μamps./mm.², fibers emerging at an angle to the current axis were deflected toward the cathode, and the authors consider that such "control of fiber growth by the current establishes one of the necessary conditions for application of an electrical theory of control and organization of the developing nervous system" (1946, p. 156) Weiss (1950) disagrees with the authors' interpretations of their results and believes that the apparent cathodal effect here also operates in part through mechanical influences.

Whatever the merits of Marsh and Beams' conclusions, there is an even more significant set of observations to be explained than those which gave rise to the theories of neurobiotaxis and stimulogenous fibrillation, namely, the demonstration by both experimental and quantitative studies that in the normal embryo outgrowing fibers grow toward regions of active mitosis (cf. Coghill, 1929; Detwiler,

1936; and Piatt, 1948), a fact which has been tentatively explained by Weiss (1934) on a mechanical basis but which still requires far deeper analysis than it has yet received. Every process occurring in an embryo, as in all other protoplasm, involves an interplay of mechanical, chemical, and electrical effects. So far as the control of nerve outgrowth is concerned, the interrelationships of these factors under physiological conditions in the embryo still remains open for analysis. While detailed investigations of the action of specific physical and chemical agents are continuously desirable and necessary, what is urgently demanded is an initiation of analysis of these interrelationships at a biological level.

The Structure of the Neurone.—The *neurone* has been, ever since its recognition as the fundamental unit of the functioning central nervous system, of great interest as a cell remarkably specialized for its function: conduction. It is, as the classical studies have recognized, a *cell-body* provided with particularly differentiated cell-processes, the *nerve fibers*. In the vertebrate, the cell-bodies are normally located in the central nervous system, with two primary exceptions: (1) the postganglionic efferent cells (see below, pp. 81 ff. for classification into functional components) of the autonomic nervous system, whose cell-bodies are located in special ganglia, and (2) the afferent neurones in general, whose cell-bodies are located usually in ganglia related to the organs of special sense, or in the spinal and cranial ganglia. There are, however, exceptions to this latter group of exceptions: the *Rohon-Beard cells*, found from fish to man (Coghill, 1914; Humphrey, 1944), which are transient afferent neurones in the embryo, have their cell-bodies located within the central nervous system rather than in peripheral ganglia.

The cell-body, as is well-known, contains the *nucleus* (usually provided with a large *nucleolus*) and the usual cytoplasmic inclusions: *mitochondria*, *microsomes*, and the *Golgi apparatus* which was first demonstrated by Golgi in the neurone (cf. Kirkman and Severinghaus, 1938, and Thomas, 1948, for modern discussions of the Golgi apparatus in the neurone). In addition, the neurone includes, in the cell-body and also in the dendrites, the specially staining *Nissl substance*; this is absent from the axone and the axone

hillock, a special cone of cytoplasm from which the axone arises. The Nissl substance may vary in different physiological conditions of the cell: e.g., it may disappear from the efferent neurones in generalized fatigue (Dolley, 1913). Its distribution in the cell is altered after severance of the axone (*chromatolysis*), which is of advantage to investigators who wish to localize the nuclei of origin of fiber tracts within the central nervous system. Various pigments are found in particular groups of cells in the brain; their significance is not yet appreciated.

The silver precipitation or impregnation methods which were introduced by Golgi and perfected by Ramón y Cajal and more recently adapted by Bodian for preparation of the nervous system for histological study have demonstrated the existence, in both cell-body and its processes, of long thin fibrils, the so-called *neurofibrils* Because of the fact that these could at first be cytologically demonstrated only in silver preparations, they have been considered for many years to be fixation-artifacts Some fairly recent tissue-culture studies (Weiss and Wang, 1936; Levi and Meyer, 1937) suggest they are structural entities within the living cell; more recently, phase microscopy has revealed in living motor neurones of an invertebrate, filaments apparently beaded in structure which seem to be identical with neurofibrils (Thomas, 1947). Uniaxial birefringence of the axone in the case of the giant fiber of the squid has been related to the existence of neurofibrils in the living condition (Bear, Schmitt and Young, 1937), as have certain linearly aggregated particles demonstrated in the same form by the electron-microscope (Richards, Steinbach and Anderson, 1943) Other studies with the electron-microscope (De Robertis and Schmitt, 1948) have demonstrated in the axones of invertebrates and of vertebrates the existence of new entities possibly tubular in nature and hence called *neurotubules* (Figure 2.2); these are fibers of indefinite length with thin dense edges, a low density in the central axis, and a characteristic periodically repeating pattern of crossbanding

The immediate and spectacular results of the application of the modern microscopic methods to the study of the neurone suggest that we would do well to suspend judgment as to the significance

of the "classical" inclusions until more results from the new studies are available. In any case, neither tubules nor fibrils can be considered to be conducting elements in the strict sense, since conduction is now known to be a membrane phenomenon.

In view of the above reservation, little, if anything, need be added concerning the cytology of the cellular processes, the *axone* and the *dendrites*. The dendrites, typically cellipetally conducting

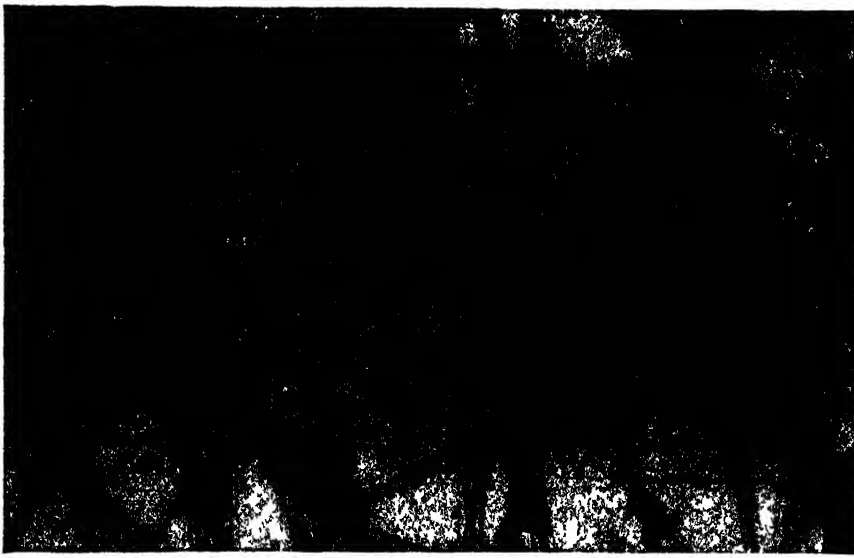


FIG 2.2. Neurotubules from the nerve axone of human sympathetic fibers, showing their characteristic dense edges and a less dense core Interganglionic segment fixed in 10% formalin, fragmented with sonic waves. Material stained with phosphotungstic acid. Magnification 22,000x (From E. De Robertis and F O Schmitt, *J. Cell Comp. Physiol*, 1948 31, 1-23; by permission of Dr. Schmitt and the Wistar Institute of Anatomy and Biology.)

processes, are often multiple in number and highly branched: the axone, typically the cellifugally conducting process, is usually single, but may branch abundantly and give off highly numerous collaterals. A word may be added about the definition of the processes in the afferent cells: with the exception of the ganglion cells of the VIII cranial nerve, which are bipolar, the afferent neurones are unipolar in the adult vertebrate, the single process branching to form a cellipetal and a cellifugal process. Functionally speaking, in terms

of the normal direction of conduction, the former is dendrite and the latter axone; structurally speaking, the single process is morphologically an axone, a set of facts which is without significance so far as nervous function is concerned but which points up a moral on the valuelessness of excessive classification and over-definition.

While the neurone proper is the actual conducting body within the central nervous system, it is appropriate to add a few words about the cellular and noncellular sheaths which may invest it. Nerve fibers may be provided with a coating of *myelin*, a lipoid-protein substance which imparts to nerve fibers their whiteness within the "white matter" of the central nervous system and in the peripheral nerves and which exhibits specific staining properties when degenerating as a result of axone severance. Studies with polarization optics and X-ray diffraction methods (Schmitt, Bear and Clark, 1935; Schmitt and Bear, 1937, 1939) have shown that the myelin sheath consists of radially oriented lipoid micellae presumably interspersed between concentric layers of protein material.

It has been conventional for some time to classify fibers as myelinated or unmyelinated. The finest and slowest-conducting fibers in the mammal, for instance, are commonly described as unmyelinated. It is now becoming apparent that this distinction may be only arbitrary, and of a quantitative nature, and that the lowest limit of the thickness of myelinated fibers or of myelin may be mainly a question of the methods applied in their study. Schmitt and Bear (1937, 1939) have recently shown that in large axones the lipoid is the predominating factor, in small ones (below 2μ) the protein. These investigators have shown, however, that fibers less than 2μ in diameter have a lipoid coat, and Brodal and Harrison (1948) consider it probable that fibers down to 0.5μ in diameter have a lipoid investment. Fibers in the sympathetic chain of mammals, formerly considered to be nonmyelinated, are now known to have a thin sheath. The possibility is not excluded, however, that some fibers may lack myelin altogether, and this question still remains open.

No specific functions can yet be unequivocally allocated to the myelin sheath. From a physiological point of view, conduction is

supposed to be fundamentally similar in myelinated and unmyelinated fibers. Because of its fatty nature the sheath has for many years been considered to act as an insulator preventing the spread of current from one conducting fiber to the adjacent ones. It has, however, recently been shown that there are circumstances under which action-currents along myelinated fibers may have their effect on adjacent myelinated fibers (Rosenblueth, 1941; Marrazzi and Lorente de Nó, 1944). The rate of conduction is in general higher in myelinated fibers than in those hitherto considered unmyelinated, but this may well be a matter of fiber diameter. It is known that many anaesthetics (e.g., alcohol, ether, etc.) are substances soluble in myelin. These are facts whose interrelationships cannot be fully appreciated until we are more certain of the specific role of myelin in the animal economy.

The cellular coat, the *neurilemma* or the *sheath of Schwann*, is found only in peripheral nerve, being assumed by many to represent the peripheral counterpart of the central glia. The sheath is deposited in segments, the interruptions between them the familiar *nodes of Ranvier* (which interrupt also the underlying myelin), and there is usually a single nucleus per segment. A considerable amount of cytological detail is available relating to the finer structure of the sheath, but further elaboration is unnecessary here in view of the lack of precise knowledge of the function of the structures involved. The sheath-cells are derived in the embryo from the *neural crest*, which gives rise also to the afferent spinal neurones and their ganglia, possibly also to some autonomic ganglia and neurones and parts of the pia-arachnoid membranes, and to a mélange of other structures (Harrison, 1937). When the neural crest is removed from embryos, the ventral roots, which are normally provided with sheath-cells, remain naked; such naked fibers apparently are able effectively to conduct nerve impulses (Harrison, 1924).

The Mechanism of Conduction by the Neurone.—The primary function of the neurone is conduction, the mechanism of which may now be examined. The role of the cell-body proper in the process still remains moot (cf. Renshaw, 1942), so the considerations below will limit themselves to a discussion of the process in

the fibers. Complete knowledge of the activity of the neurone in conduction must involve an analysis of the nature of (1) the action of the excitatory stimulus, (2) the initiation and transmission along the nerve fiber of the propagated wave of electrochemical change which is the nerve impulse, and (3) the transmission of the impulse from one neurone to another. The last of these three topics will be considered below in connection with the discussion of the synapse, and the present section will confine itself to a discussion of the second of the topics.

Just a little over a century ago Johannes Muller resigned himself to the conviction that the time required for transmission of a sensation from the periphery to the brain is "infinitely small . . . and unmeasurable" (quoted from Amberson and Smith, 1940, p. 123); only six years later it was measured with reasonable accuracy by Helmholtz. The heat production of nerve, which is so low that for many years it went undetected and was believed not to exist, was measured at last by A. V. Hill in 1926. It has been calculated that the energy required for the propagation of the impulse is less than one-tenth of a millionth of a small calorie per impulse per gram of nerve. Respiration is commensurately low, but has proved measurable, and there are in course a great many studies on the respiration of nerve.

The *nerve impulse* may be defined as a propagated wave of electrochemical change. The historical controversy as to whether the electrical or the chemical aspects of conduction are primary has lost its significance with the recognition of the fact that both types of activity are involved in the production of a single integrated mechanism. The masterly technical achievements of the workers who have devoted themselves to the study of the electrical aspects of conduction have made possible the amplification, recording, and what is more important, the measurement of the change of electrical potential which accompanies the propagation of the nerve impulse, with a degree of precision which is one of the more spectacular feats of modern science; and indeed, the ingenuity of their analyses is being rivaled at present only by the elegance of the studies on enzyme activity in the nervous system which are elucidating the story on the chemical side.

Conduction of the nerve impulse has been for many years considered to be a membrane phenomenon (cf. Lorente de Nó, 1946, for modern discussion), related to the fact that the membrane is electrically polarized in a resting fiber (Figure 2.3). The action of the stimulus is to depolarize the membrane in such a way that a potential difference, of the magnitude of about 25 to 50 millivolts as measured externally, is established between active and inactive regions. The depolarized region becomes negative to the adjacent portion, a flow of current from polarized to nonpolarized region being produced, the *action-current*, which stimulates the next fol-

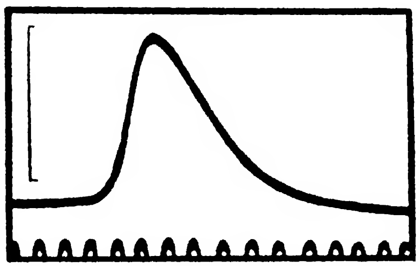


FIG. 2.3. A monophasic potential wave, the type usually recorded for study, produced by placing the distal electrode on an injured region of the fiber (From A. L. Hodgkin, *Proc. Roy. Soc.*, Series B, 1938, 126, 87-121, by permission of the author and The Royal Society of London.)

lowing region, resulting in a spread of the depolarization, and at the same time permitting a restoration of the polarized surface after the wave passes. Because of the existence of a refractory period, the restored surface cannot immediately become depolarized again, with the result that the area of depolarization is propagated along the fiber, the potential change at any one point resulting in the depolarization of an adjacent section. The potential change is thus an essential link in the transmission of the impulse.

The polarization of the membrane postulated by this theory has been believed for some time to involve a differential movement of ions, and to be related to a selective permeability of the axone surface to the potassium ion. During rest, the concentration of potassium is higher within the axone than without. During activity, a change in membrane permeability has been assumed as responsible for the disappearance, or even reversal, of the resting potential.

The recent studies by Nachmansohn and his co-workers (1945, 1946a, 1946b) present evidence for the fact that the appearance and removal of the ester *acetyl choline* are responsible for the change in permeability; according to this theory, acetyl choline, produced intracellularly, is released at the point of depolarization, its rapid destruction by the enzyme *cholinesterase* being responsible for the restoration of the resting membrane potential. The evidence available from these studies suggests that acetyl choline acts to increase the permeability of the membrane by decreasing resistance. On the basis of a.c. impedance measurements on the giant axone of the squid, it has been interpreted that during the passage of the impulse the resistance drops from 1000 to 25 ohms per square centimeter.

One of the early objections raised to this theory was related to the factor of time (cf. Eccles, 1937). The high speed of propagation of the impulse (a maximum speed of 120 meters per second has been described) seemed to some to render it dubious that a purely chemical reaction could be at the basis of it. The high speed at which enzyme reactions are known to occur removes this objection, and it is now established that the metabolism of acetyl choline is as rapid as the conduction of the electrical impulse would necessitate. It has been shown, for instance, that in mammalian brain 10^{14} to 10^{15} molecules of acetyl choline may be activated per gram of tissue per millisecond. In further support of the theory, it is known that in the nerve fiber cholinesterase is localized in the surface of the axone, in a layer probably only a few micra thick, in contrast to the respiratory enzymes which are found primarily in the axoplasm, the central core of the axone. Inhibitors of cholinesterase, for instance eserine, abolish the nerve action potential according to Nachmansohn and his co-workers. Evidence is also accumulating for the existence of another enzyme, *choline acetylase*, so far found only in nerve tissue, which is presumably responsible for the resynthesis of acetyl choline, and the energy for resynthesis is believed to be derived from the energy-rich bonds of adenosine triphosphate.

It may be remembered that acetyl choline was first postulated to act as a neurohumoral or synaptic transmitter secreted at the nerve ending and acting there only. No less an authority on the

electrical aspects of conduction than Gasser (Erlanger and Gasser, 1937) had drawn an analogy describing the spikes on the electrical recordings as comparable to the ticks of a clock, as indicative rather than productive of activity. According to the present theory, the release and removal of acetyl choline along the whole length of the neurone is the mechanism responsible for the alteration of the membrane during conduction, and hence for the flow of current. This new concept assumes that the transmitting agent is the electrical current, the release of acetyl choline being necessary for generating the current.

Considerable doubt, however, has been cast on the validity of this hypothesis by the recent work of Lorente de Nó (1946, 1947, 1949). This investigator has been able to show experimentally that the value of the membrane potential is not dependent on the ratio of the internal and external concentrations of potassium, and that the membrane potential is not maintained by outward diffusion of potassium ions According to his interpretation, the assumption is unnecessary that the nerve membrane is semipermeable.

He has questioned also the validity of the hypotheses concerning the action of acetyl choline, having shown experimentally that neither acetyl choline nor eserine necessarily depolarizes nerve fibers nor prevents the conduction of impulses. In the case of synaptic transmission in the spinal cord, at least, he has interpreted his experimental results as demonstrating that the release of acetyl choline is a consequence of the metabolic activity that accompanies and follows the production of the impulse He admits the assumption that acetyl choline is a substance participating in the metabolic activity of the fiber, but considers it to play a secondary rather than an immediate role in the production of the impulse. It is his belief that the membrane potential is directly established by the oxidative metabolism of the cell, and that applied currents modify the state and properties of the membrane because by changing the value of the membrane potential they alter the course of reactions of the oxidative chain.

On the whole, the postulation of a chemical basis for the production of action-currents enhances rather than otherwise the results

of the remarkable studies that have been made on the electrical aspects of conduction, and to these latter we may now turn attention (more specific references to this work may be found in the reviews by Gasser, 1937, 1941, and in the book by Gasser and Erlanger, 1937)

The speed of conduction has been measured with remarkable accuracy It is now known to vary, in general, with the size of the fiber in large axones, for instance, the motor fibers in the mammal which are about 20μ in diameter, the speed of conduction can attain a rate of 100 to 120 meters per second, in fibers 1μ or less in diameter the rate may be as low as 0 3 meter per second A mixed nerve, like the mammalian sciatic, carries fibers conducting at different rates Action-currents in mixed nerves fall into three groups, the so-called *A*-, *B*- and *C-waves* The A-waves the most rapid, are characteristic of the larger motor and sensory fibers the latter being those particularly concerned with touch, pressure, and proprioception The B-waves are slower and presumably arise from smaller medullated fibers responsible for sensitivity to heat and to localizable pain and from preganglionic efferent fibers of the autonomic system The C-group are slowest, these characterize the fine fibers hitherto considered unmyelinated which are responsible for diffuse pain, and also the fine fibers of the autonomic system formerly considered to be nonmyelinated

The relationships to conduction rate of the nodes of Ranvier, and the consequent interruption of myelin and cellular sheath, have been subjected to some analysis but are still moot Conduction occurs without decrement, i e , the currents do not decrease in magnitude as they are propagated along the course of a fiber The magnitude of the impulse, furthermore, remains the same no matter what the strength of the stimulus, provided this reaches at least threshold value (this is the so-called *all-or-none-law*) An increase in the strength of the stimulus, while it does not increase the magnitude of the impulse, may result in an increase in the number of units called into action, and does increase the frequency of impulses: the maximum frequency computed as possible is 2500 impulses per second, in the rapidly conducting mammalian motor A-fibers. The maximum frequency is determined by the length of the refractory

period; in these fibers, there is an absolute refractory period of 0.4 to 0.44 millisecond duration during which no conduction occurs, and a relative refractory period of 3 milliseconds, during which the threshold for stimulation is higher, and the size of the impulse smaller, than normal. These in turn are followed by recovery periods of supernormal (15 milliseconds) and subnormal (87 milliseconds) excitability during which the threshold also deviates from normal. The duration of the spike which indicates the period of maximum negativity is 0.4 to 0.43 millisecond in recordings from A-fibers, about 2.5 milliseconds in fibers which conduct more slowly.

The threshold for stimulation varies for fibers of different size, being lower for the larger and faster-conducting fibers. While subthreshold stimulation does not initiate the passage of an impulse, it may produce a change within the fiber (*local excitatory state*) which may be responsible for the fact that a second subliminal stimulus, at the proper time, can evoke the production of an impulse; this is *summation* one possible mechanism of *facilitation*, to be discussed further below, and it is not impossible that the action of currents affecting adjacent fibers, described above on page 55, may be involved in such facilitation

It seems clear, from all that is known about the kind and magnitude of change that occurs during its propagation, that the nerve impulse is fundamentally uniform for nerve fibers of all varieties in the vertebrate The differences in quality of sensations are related not to differences in nerve impulses, but to the fact that the different sense organs are differentially selective in the type of stimulus to which they respond and to the reactions of central areas. The differences in reactions are related ultimately to differences in the effector organs. There are, in the vertebrate, however, invariably at least two neurones, and almost always more than two, interposed between sense organ and effector. Our appreciation of the total mechanism of nervous action depends therefore on our knowledge of the relationships between neurones; as Gasser has put it (1937, p. 173): "The laws describing the mode of transmission of excitation across a synapse . . . must be the basic laws of the nervous system." To the synapse, therefore, we may next devote our interest.

Interrelationships Between Neurones: the Synapse

Morphological Properties of the Synapse.—It is one of the peculiarities distinguishing the vertebrate nervous system that transmission of an impulse from one neurone to the next occurs within the limits of the central nervous system proper, brain or spinal cord: the main exception to this rule is in the case of the transmission from preganglionic to postganglionic fibers of the autonomic system which occurs in autonomic ganglia. The junction between one neurone and another is defined as a *synapse*. The essence of the neurone doctrine is its postulate that neurones are separate units, not *continuous* with one another. The essence of the synapse is that it is a region where neurones are *contiguous* with one another, and it is by virtue of this contiguity that the transmission of the impulse from one unit to the next takes place A synaptic membrane has been described as a visible entity in many cases, but there is no reason to expect uniformity of structure at all synapses (cf. Bodian, 1942).

The axonal endings at the synapse (Figure 2.4) take the form of irregular bulbous expansions, called usually by their French name of *boutons,* which vary in dimension roughly from 0 5μ to 7μ. Following sectioning of the axone, the boutons exhibit a peculiar and readily recognizable form of degeneration which has been of great assistance to experimental neurologists tracing fiber-tract connections.

The high degree of diversity which characterizes the synaptic relationships between neurones cannot be overemphasized. The end-to-end synapse, which typifies the synapse in so many textbook diagrams, is perhaps in actuality the rarest type in the nervous system. The axonal endings of one cell may, however, be highly ramified among complexly branched dendrites. A complex and intricate interramification of axone endings and telodendria may constitute a large part of the nonorganized gray matter of the central nervous system, the neuropil, which according to some (Herrick, 1948) may be highly significant in central integrative processes.

The frequent and prevalent textbook emphasis on the synapse as a region of communication between the axone of one neurone and

the dendrite of the next is likewise erroneous and misleading. In probably the greatest number of cases the axone terminates on the cell-body, rather than among the dendrites, of the related neurone; it may even end in the region of the axone-hillock, or, curiously enough, on the proximal portion of the axone itself. Each axone, through its collaterals or branches, may have relationships with many other neurones. Conversely, neurones may receive termina-

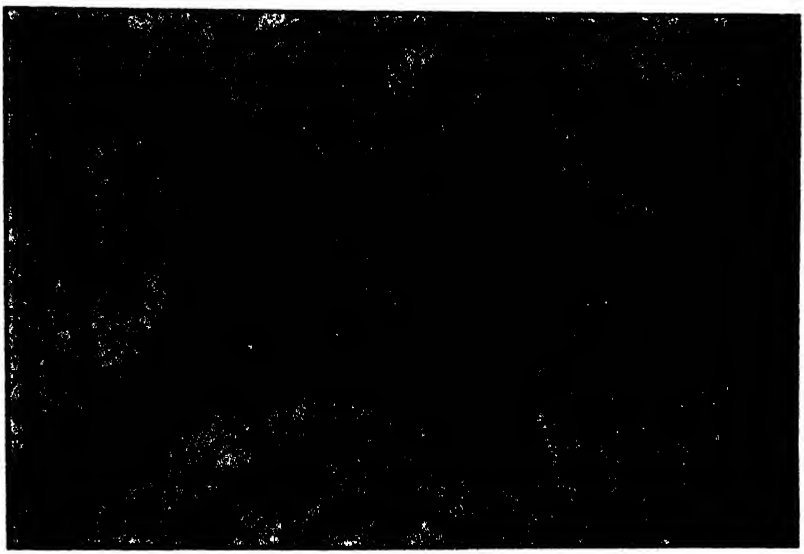


FIG. 2.4. Drawing of section through ventral horn cell in cervical region of cat's spinal cord Magnification approximately 750 x Note the large number of end bulbs on surface of cell body and proximal portion of dendrites (From M L Barr, *J. Anat.*, 1939, 74, 1 11, by permission of the author, the Editors of the Journal of Anatomy, and Cambridge University Press)

tions from many other cells (Figure 2.4): single ventral horn cells in the mammal have been calculated to receive well over a thousand synaptic endings, and it has been claimed that at least 38 per cent of the surface of the ventral horn cells in mammals may be covered by synaptic endings (Barr, 1939). Hundreds of synaptic junctions may occur between a single pair of cells. Two types of neuronal circuits have been described (Lorente de Nó, 1938c), a multiple chain and a closed chain (Figure 2.5) which allow for the reinforcement of stimulation, for prolonging the period of discharge, and

for much increase in flexibility of action The significance of the demonstration of a morphological basis for a plurality and reciprocity of connections cannot be overemphasized, and the multiple and closed chains of neurone circuits are now receiving great emphasis as integrative mechanisms

Transmission at the Synaptic Junction.—It was first postulated, as early as 1904 (Elliot, 1904), on the basis of the similarity of action of adrenaline to stimulation of the sympathetic nervous system, that sympathetic nerve endings might act by liberation of a substance comparable to adrenaline This was strongly supported by Loewi's demonstration in 1921 that a substance, the *Vagusstoff*, later identified as *acetyl choline*, appeared in the perfusion fluid

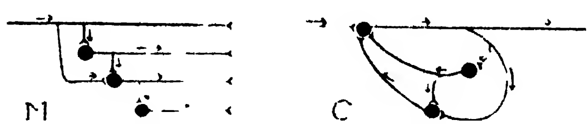


FIG 2 5 Plans of the two fundamental types of neurone circuit M, multiple chain, C, closed chain (From R Lorente de Nó, *J Neurophysiol* 1938 1, 207-244 by permission of the author and the Editors of the Journal of Neurophysiology)

following stimulation of the vagus in a frog's heart preparation, which affected an isolated second heart in the same way as vagus stimulation The results of these and comparable studies gave rise to a more general concept of neurohumoral transmission (cf Dale, 1937), whereby the nerve ending was presumed to act by the secretion of a specific chemical substance or substances A great many investigations followed, which suggested acetyl choline to be the transmitting substance at parasympathetic nerve endings and a substance related to adrenaline at sympathetic endings

The recent studies, reported above (Nachmansohn, 1945, 1946a, 1946b), on the role of acetyl choline in conduction along the length of the axone have necessitated a modification of the original neurohumoral hypothesis, which had postulated that transmission across the synapse differed fundamentally from transmission along the course of the axone. According to Nachmansohn' concept, the role of acetyl choline is the same in transmission o the impulse along the axone and across the synapse; in both cases alike, the

transmitting agent, *per se*, is the electric current; the release of acetyl choline is necessary for generating the current. The principal differences between propagation along the fiber and across the synapse are described as quantitative ones, rather than qualitative. And this fact may hold whether Nachmansohn's or Lorente de Nó's interpretation of the mechanism producing the impulse approximates closest to the conditions obtaining. In either case, it is these differences to which some of the physiological properties of the synapse may relate, and to these we may now proceed.

Physiological Properties of the Synapse.—Granted that the nerve impulse is a propagated wave of electrochemical change, whether or not the modern hypotheses are accurate as to the nature of its genesis and as to its fundamental likeness along the fiber and at the synapse, yet it is clear that the synapse imputes some properties to conduction through a series of neurones that are not rendered apparent by analysis of conduction in a single fiber. It is appropriate now to examine some of these properties.

Obviously the first of these is polarity A nerve fiber can conduct in either direction; this is self evident from a consideration of the manner of propagation of the impulse Under ordinary circumstances, in the whole organism, a nerve fiber conducts only in one direction, although there may be exceptions to this rule in the case of so-called *antidromic conduction*. The vasodilatation which occurs as a result of dorsal root stimulation, the so-called *axone reflex*, perhaps responsible for the "flare" reaction in inflammation, is considered possibly to involve such antidromic conduction (Lewis and Marvin, 1927), although the possibility is not yet excluded of the existence of some efferent vasomotor fibers in dorsal roots which might account for this phenomenon (discussed fully in Fulton, 1943, pp. 28-29). But while antidromic conduction is possible, it is unusual, and the typical unipolarity of conduction is imparted to the conduction routes in the functioning central nervous system by the synapse, which allows conduction only from an axone of one neurone to the cell-body or the dendrites of an adjacent one, never in the opposite direction (Sherrington's *law of forward direction*). It is possible that the irreversibility of conduction at the synapse

may be explained on a purely quantitative basis. At the axone ending, where the surface increases as a result of multiple branching and as a result of the expansion of the endings to form the boutons, there may be sufficient decrease in resistance to allow a sufficient flow of current to cross the nonconducting gap. The large number of small terminals may each provide a stimulus which is subliminal, yet which in the aggregate may summate sufficiently to excite the cell-body or dendrites; whereas the latter fail to develop potential differences of high enough intensity and long enough duration to excite the synaptic axone terminals.

Another characteristic of conduction which differs quantitatively at the synapse and along the fiber is the rate of propagation, and, as is well-known, the transmission of the impulse across the synapse involves a delay which, although small (it may be as low as 0.5 millisecond) (Lorente de Nó, 1938a), is of significance in the analysis of conduction along so-called reflex-arcs. Synaptic delay can almost certainly be related to the fact that the branching terminal fibers of the axone are far smaller in diameter than the main fiber of the axone proper, and hence conduct more slowly: in other words, the apparent "synaptic" delay may occur rather in the fibers leading up to the synapse than in the synapse itself. A factor limiting precise analysis of this effect is the extreme tenuosity of the fine fibers as they approach the synapse, which makes it difficult to ascertain their exact length. For more specific and detailed discussion of the modern views on synaptic transmission the reader is referred to the work of Lorente de Nó (1935, 1938a, b, c, 1939), to Fulton (1943, 1946), and Lloyd (1946).

Whether or not these peculiarities of conduction, and others which are supposed to distinguish conduction across the synapse from that along the fiber proper (e.g. greater fatiguability, differences in metabolism, differences in susceptibility to drug action, etc.) can be explained completely on such quantitative bases remains to be seen. In any case, however, no matter how produced, these physiological attributes of the synapse are responsible for some of the known phenomena characteristic of the action of the nervous system as a whole, and are highly significant for the function of the vertebrate nervous system both so far as it is characterized by flexi-

bility allowing great freedom of individual action, and by its ability to integrate these separate activities into a well-ordered pattern of behavior.

The Synapse as a Central Regulatory Mechanism.—For a nerve fiber, appropriately stimulated, conduction is obligatory. Not so at the synapse: here transmission does not so necessarily follow stimulation: as Lorente de Nó has stated, "figuratively speaking, synaptic transmission is 'optional' for any neurone" (Lorente de Nó, 1939, pp. 402-403). Whether or not a particular neurone will respond to synaptic stimulation may well depend on its own particular relationships to the axone terminations related to it, and these relationships may now be further considered from a functional point of view.

The action of subliminal stimuli in the production of the so-called local excitatory state, which facilitates the evocation of an impulse by another subliminal stimulus, has its counterpart at the synapse. A discharge at a single axone terminal may be insufficient to excite the cell, yet may have its effect on it; Sherrington postulated (cf. Sherrington, 1929, for discussion) some time ago the existence of a so-called *central excitatory state* to account for the condition where a cell is altered by subliminal stimulation at the synapse.

It is now believed by many that the known activities of the neuronal endings which impinge on the cell to be excited are sufficient to explain the phenomenon (Lorente de Nó, 1935, 1938a, b, c). The question arises here, however, in contrast to considerations concerning the fiber, as to the possibilities of *temporal* versus *spatial summation* as mechanisms of facilitation. There has been some experimental evidence presented which has suggested in the past that *temporal summation* can occur at a single synapse. This is, however, of more theoretical than practical interest, since the minimum interval at which inadequate stimuli must be repeated in order to be effectively summated proves to be less than the refractory period of the afferent neurone. Accordingly, *spatial summation* is now believed to be of primary significance at the synapse. Summation effects can be clearly demonstrated when successive or simultaneous stimuli are applied to different afferent nerves discharging into the

same center. *Convergence* of many afferent endings and associated overlapping (Sherrington, 1929) of their fields on the individual neurone or on neurone pools receiving endings from different afferent conductors are thoroughly adequate to explain spatial summation.

Occlusion, and *inhibition* as well, also can and have been accounted for by temporal and spatial synaptic effects (Sherrington, 1929). The phenomenon is described as *occlusion* when a summated response to stimulation of several afferent conductors is less than the sum of the individual responses: this too is explained by the fact that the efferent neurones involved receive an overlap in nerve supply.

The fact that nerves can inhibit as well as excite has been known ever since 1845 when the Weber brothers rediscovered that stimulation of the vagus resulted in temporary cessation of the heartbeat; and the property of *inhibition* has seemed one of the most dramatic and mysterious of nervous activities. Many theories have been elaborated to account for it, none of which, however, is completely satisfactory, and the precise nature of the mechanisms responsible for the process still remains obscure

It is now accepted that for the somatic components of the nervous system in vertebrates the process is central rather than peripheral, but more than this is uncertain The old Wedensky theory of interference of impulses, fortunately deleted at last from the textbooks, is agreed upon as inadequate; there is no evidence to support the hypothesis that a particular depressant substance is produced in the central nervous system.

Inhibition has, however, certain properties similar to those of excitation, and Sherrington had postulated the existence of a *central inhibitory state* analogous to the *central excitatory state*. The modern tendency is to attempt explanation of the former, like the latter, on the basis of known facts concerning transmission of impulses along and between neurones. There are some who believe that the absence of facilitation at the synapse may be sufficient to account for inhibition. Others (Renshaw, 1941; Lloyd, 1941 1944) consider that it results from direct polarization of the cell-body by

adjacent neurones (direct inhibition) One of the most promising of recent theories is that of Gasser (1937) which relates the phenomenon to the subnormal period of excitability discovered by Graham (1935). He has postulated a mechanism (sometimes called indirect inhibition), illustrated in Figure 26, whereby if two afferent pathways converge at the internuncial level, the subnormality of the internuncial neurones common to them both could result in an inhibition of one by the other Gasser himself has emphasized that his

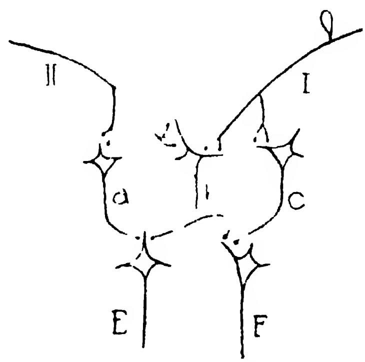


FIG 26 Diagram explaining reciprocal innervation of flexor and extensor muscles It is assumed that threshold stimulation of neurones with normal threshold requires simultaneous activation of two synaptic knobs, but three knobs are required to stimulate a neurone having subnormal threshold When fiber I conducts a rhythmic series of impulses at low frequency, neurone F is stimulated by the b and c impulses and the flexor muscle contracts, but if fiber II then becomes active cell b will be forced to discharge an extra impulse and to acquire subnormal threshold Henceforth cell b will be able to respond only to the impulses conducted by fiber II, the result being that the extensor muscle contracts, while the flexor muscle relaxes because the b and c impulses reach neurone F at intervals longer than the period of effective summation of impulses delivered at neighboring synapses (After H S Gasser, *Harvey Lectures*, 1937, 32 169-193, by permission of the author and The Harvey Society of New York)

scheme should not be taken as a complete explanation of inhibition, but even if it is only partially explanatory, it is of great theoretical interest in focussing attention once more on the fact that the kind of knowledge we are already developing concerning transmission along the fiber and across the synapse may prove adequate, ultimately, to explain much that has seemed obscure in the more complicated processes of nervous action

These postulated explanations of facilitation, inhibition, and so forth, may well be oversimplifications, but they have another theo-

retical and practical significance, also, in that they point up the importance on the physiological side of the chains and circuits of neurones whose significance on the morphological side was pointed out above. As early as 1911, Ramón y Cajal described chains of neurones in the olfactory system so arranged that the impulse seemed to him to accumulate energy as it proceeded: many peripheral fibers of the olfactory neurones, for instance, discharge into one or two mitral cells, allowing for summation of stimuli in the latter; collaterals from the olfactory tracts, through the granule cells, carry the discharge from the mitral cells back again to the mitral cells, thus reinforcing their discharge. Ramón y Cajal (1911) called the results of such reinforcement of stimuli by the name of *avalanche conduction*. The two fundamental types of neuronal circuits, the multiple chain and the closed chain, postulated by Lorente de Nó, as described above, and shown in Fig. 2.5 of this chapter, allow similarly for continuation and repetition of barrages of stimuli at single cells or at pools of cells, and such circuits, now known to be characteristic of many parts of the central nervous system, especially the cerebral cortex, are being vigorously analyzed It becomes readily apparent, when the multitudes of possible interconnections between central neurones are considered, that such multiple chains of neurones, and pools of neurones, may be of far greater significance for normal nervous action at the integrative level than the conventional and classical so-called reflex-arc

Interrelationships Between Neurones: the Reflex-arc

The Definition and the Components of the Reflex-Arc.—The concept of reflex action goes back at least as far as Descartes (who did not, however, himself use the term). Though many other experiments, at the hands of many investigators, contributed to the elaboration of the theory of the reflex-arc, its definition as the functional unit of the nervous system ensued rapidly upon the experimental demonstrations by Bell and Magendie that the dorsal spinal roots in the vertebrate are sensory and the ventral are motor in function. The materialistic nineteenth century, grateful, no doubt, for a visible concrete model which could account for some of the

apparently mysterious actions of the central nervous system, welcomed the theory, and it was probably with comfort that Marshall Hall could write in the 1830's that the spinal cord is a chain of segments whose functional units are separate reflex-arcs, which interact with one another and with the higher centers of the nervous system to secure coordinated movement. There are few textbooks concerned with action of the nervous system which do not still today bear a diagram of the segmental two- or three-neurone reflex-arc, composed of an afferent fiber carrying toward the central nervous system an impulse which is there transmitted directly, by a single synapse, or indirectly, through the mediation of an internuncial neurone, to the efferent neurone which innervates the ultimate effector.

It is an irony of contemporary scientific history that Sherrington's monograph of 1906, which analyzed exhaustively the segmental reflex with the express purpose of demonstrating its integrative function, has focused the thinking of the current century, at least at the pedagogic level, on the segmental arc as an individual and separate unit

> "The reflex-arc is the unit mechanism of the nervous system," the twentieth century repeats with Sherrington, but it often forgets to add the words which immediately follow, "when that system is regarded in its integrative function . . . The nervous synthesis of an individual . . . resolves itself into co-ordination by reflex action. But though the unit reaction in the integration is a reflex, not every reflex is a unit reaction, since some reflexes are compounded of simpler reflexes. Co-ordination, therefore, is in part the compounding of reflexes . . . There is the co-ordination which a reflex action introduces when it makes an effector organ responsive to excitement of a receptor, all other parts of the organism being supposedly indifferent to and indifferent for that reaction In this grade of co-ordination the reflex is taken apart, as if separable from all other reflex actions This is a *simple reflex*. A simple reflex is probably a purely abstract conception, because all parts of the nervous system are connected together and no part of it is probably ever capable of reaction without affecting and being affected by various other pa s, and it is a system certainly never absolutely at rest. But the simple reflex is a convenient, if not a probable fiction" (Sherrington, 1906, pp. 7-8).

And again later:

> "We have hitherto dealt with reflex reactions under the guise of a convenient but artificial abstraction,- -the simple reflex. That is to say, we have fixed our attention on the reaction of a reflex-arc as if it were that of an isolable and isolated mechanism, for whose function the presence of other parts of the nervous system and of other arcs might be negligible and wholly indifferent. This is improbable. The nervous system functions as a whole. Physiological and histological analysis finds it connected throughout its whole extent. . . . A reflex detached from the general nervous condition is hardly realizable. . . The compounding together of reflexes is therefore a main problem in nervous co-ordination" (Sherrington 1906, pp. 114-115).

His warning could not have been clearer, nor less heeded.

The theoretical reflex-arc presupposes then a series of components whose anatomical and functional interrelationships demand some description The essential components of the arc are at a minimum three, if we include the effector organ innervated. There is always an afferent nerve, either itself directly, or through the intervention of an organ of special sense indirectly, excitable as a result of some energy-change which is the stimulus The afferent nerve transmits its impulse across a synapse in the central nervous system to the efferent neurone, which in turn stimulates the effector organ responsible for overt response. As already implied on page 61, it is questionable to what extent two-neurone pathways exist within the vertebrate nervous system; certainly in by far the great majority of cases, if not in all, at least one so-called internuncial neurone is interposed between afferent and efferent neurones.

The anatomical structure of the afferent neurone is alone sufficient to render untenable the theory that a strictly segmental reflex can function in an isolated manner in the intact organism. The cell-bodies of these neurones in the adult vertebrate are located, it will be remembered, in peripheral ganglia Their centripetal processes enter the central nervous system either with the cranial nerves or in the dorsal roots of the spinal; in the spinal segments they branch immediately upon their entrance into the cord into a longer ascending

and a shorter descending branch. Each of these may give rise to a vast number of collaterals, each of which may be stimulated by every impulse conducted along the primary fiber. The ascending fibers may reach the medulla oblongata, or may terminate, as do some of their collaterals and as do the descending branches and their collaterals, in the ipsilateral or contralateral gray of the cord at levels near to or remote from the level of entrance. Recognition of their multiplicity of subdivision, suggesting therefore a commensurate multiplicity of possible functional connections, should be ample to convince the most prejudiced isolationist of the appropriateness of Sherrington's warnings.

If a multiplicity of relationships with other cells is a remarkable property of the single afferent neurone, it is none the less outstandingly characteristic of the efferent neurone. The profusion of synaptic endings on a single ventral horn cell has been described and illustrated above (Figure 24), and it remains here only to emphasize the fact that these synaptic endings may derive from tracts which vary in function, and to point out the significance of this fact for the so-called reflex-arc as an integrative mechanism.

In the first place, the dendrites of the ventral horn cells ramify widely through the cord, thus presenting a wide and scattered surface over which impulses may enter. In the second place, that motor horn cells receive endings from various afferent cells within the mammalian cord is implicit in the fact that there are a greater number of afferent than efferent cells; Donaldson (cited in Sherrington, 1906, 1947) has calculated that for the spinal cord of man the afferent neurones outnumber the efferent by a ratio of three to one, and Sherrington (1906, 1947) considers that if the cranial nerves were included the ratio would be five to one. Finally, as a result of the usual methods of establishing fiber-tract connections, the cell-body of the ventral horn cell is known to receive endings not only from afferent fibers entering ipsilaterally and contralaterally at its own and at other levels, but also from many tracts descending from the brain; in the human, for instance, from the corticospinal tracts from the cortex, the rubrospinal from the red nucleus relaying impulses from the cerebellum, the tectospinal from the midbrain, the olivo-

spinal, the reticulospinal, and the vestibulospinal from the medulla oblongata, and probably others as yet unidentified.

The ventral horn cell, then, acts as what Sherrington has called the *final common pathway*, "the *sole* path which all impulses, no matter whence they come, must travel if they are to act on the muscle-fibers to which it leads" (1947, p. 117). Whereas the afferent neurones are in a sense selective and private paths, each being excitable through its own endings directly or through an interposed sense organ indirectly, by a specific kind of stimulus, the efferent neurone is, in Sherrington's words, "a public path, *common* to impulses arising at any of many sources of reception" (*loc cit.*). Sherrington, again in an analogy which hardly bears improvement, likens the action of the motor unit as the final common pathway to that of a funnel: "the receptor system bears . . . to the efferent paths the relation of the wide ingress of a funnel to the narrow egress" (*ibid.*, p. 148). The funnel-like action of the final common pathway is one of the primary and fundamental mechanisms whereby what might otherwise be local, separate and isolated action becomes integrated with other such actions into the coordinated behavior of the organism as a whole.

The Reflex-Arc and the Total Behavior Pattern in Development.—Partly, probably, because investigators frequently indulge the hope that a solution of an apparently simple problem for the embryo may lead to the resolution of more complicated ones for the adult, the relationship of the so-called reflex-arc to what may be called the total behavior pattern is being actively investigated for the embryo. Since the key work on the problem was carried out by an investigator who undertook his analysis with the major problems of behavior in mind, and since much of the subsequent work has been continued by investigators whose primary fields of interest are psychological in scope, it seems relevant to discuss briefly some aspects of this work here in relation to the larger issues we have been considering.

Coghill, who began the original work in this field, undertook, in his own words, "to analyze behaviour by studying its development, with the thought in mind that such an analysis should con-

stitute a new approach to fundamental problems concerning the function of the nervous system and its parts, and that anatomical studies correlated throughout with the behaviour studies should throw a new light upon the problem of behavior" (Coghill, 1929, p. 1). Accordingly, he carried through an inspired and painstakingly accurate program of studies on the development of behavior in the salamander, and on the development of the morphological changes in the nervous system which accompany the maturation of the functional pattern

He pointed out that in *Amblystoma*, the form under consideration, a regular series of stages occurs during the development of the adult pattern: (1) a nonmotile stage during which the muscles are excitable only by direct mechanical or electrical stimulation, and not by any stimulus whose transmission involves the intervention of the central nervous system; (2) an early flexure stage, when the animal responds to slight stimulation of the skin by a localized contralateral contraction of the anterior trunk musculature; (3) a coil stage, during which more segments are involved in the contraction; (4) an S-reaction stage, during which the early flexure is reversed before being completely executed into a coil; and finally, (5) a stage at which the repetition of the S-reaction occurs in such a way that locomotion is accomplished (Figures 2.7, A, B and C).

One of Coghill's fundamental contributions was the demonstration of the development of the specific mechanism, on the anatomical side, by which the first phase described becomes transformed to the second. He established the presence, during the first stage, of the sensory cells, the Rohon-Beard cells, which receive impulses both from the skin (exteroceptive) and from muscle (proprioceptive), and of the motor cells in the ventral gray matter of the cord which innervate the muscle segments which constitute the longitudinal musculature whose contractions are ultimately responsible for the swimming movements of the animal. He showed further that during the nonmotile stage, although a central pathway is already established by which the sensory cells might be expected to conduct headward, and a motor pathway by which the motor cells might be expected to conduct tailward, yet no mechanism of central commu-

nication, direct or indirect, is present to allow transmission from one of these systems to the other.

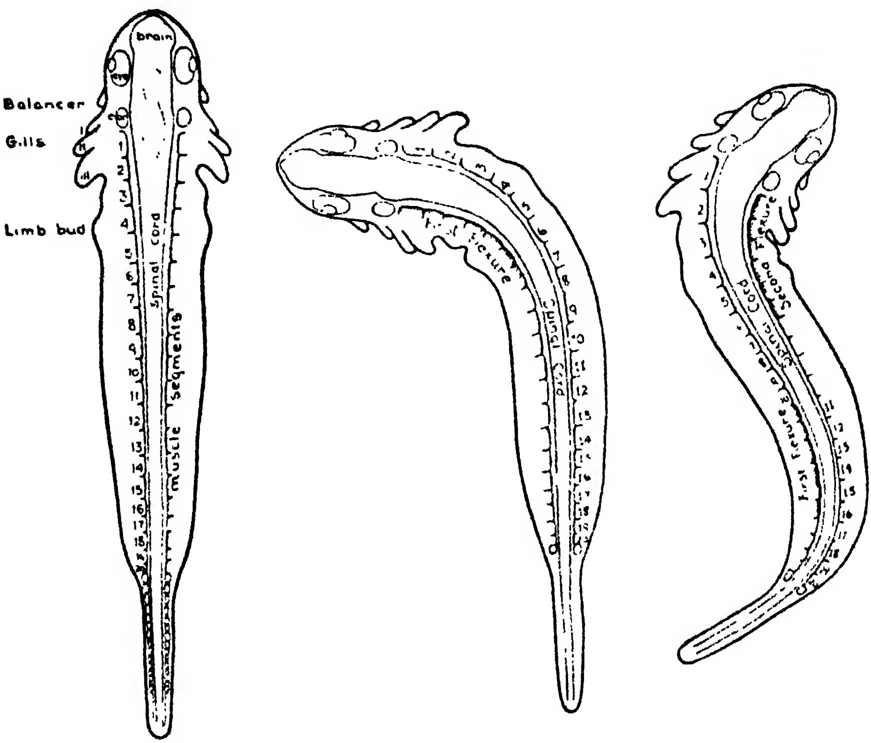
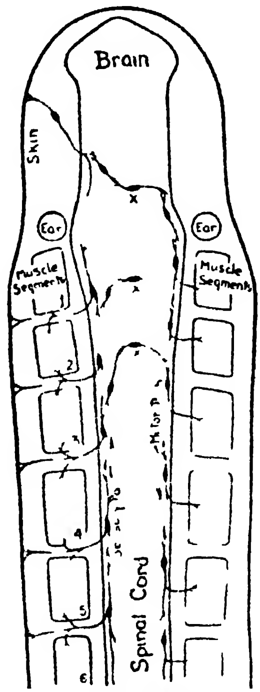


FIG. 2.7. *Amblystoma punctatum* embryos at the early swimming stage. A, a diagram of an embryo at the early swimming stage, made to scale from serial sections to show important structures as seen from the dorsal side. The arrangement of the muscle segments alongside of the spinal cord and the absence of limbs excepting very early primordia of the fore limbs are important features. Actual length of the embryo approximately 7 mm. B, a diagram modified from that of 13A to illustrate the beginning of a swimming movement as a first flexure by contraction of a number of anterior muscle segments, indicated by cross hatching. C, a diagram to illustrate the swimming movement in which the first flexure has passed tailward and the second flexure is beginning in the anterior region by contraction of the muscle segments indicated by cross-hatching. (From G. E. Coghill, *Anatomy and the problem of behaviour*, 1929, Macmillan Co., New York; by permission of The Macmillan Company.)

This condition changes at the time of onset of the response to skin stimulation by muscular reaction. At this time the first internuncial neurones are completed, as commissural cells in the floor-

plate of the medulla oblongata and anterior cord (Fig. 2.8). These had been present at the nonmotile stage, but unipolar, in relationship



x = Floor Plate Cells

FIG 2.8 A diagram of the mechanism which accounts for the coil reaction, cephalocaudal progression of movement and movement away from the side stimulated in *Amblystoma*. A stimulus in front of the ear would excite an afferent neurone, which, in turn, would excite a commissural cell in the floor plate. This cell turns caudad in the motor tract of the other side. A stimulus behind the ear, whether in the skin (exteroceptive) or in the muscle (proprioceptive) excites the afferent neurones which also excite the commissural cells of the floor plate, through which the impulse reaches the motor tract of the other side. Since the afferent system sends impulses to the motor system of the opposite side through floor plate cells and since the floor plate cells form a commissure only in the brain and anterior part of the spinal cord at this stage, impulses excited from any point in the body, whether headward or tailward, from the skin or from the muscles must enter the neuromotor mechanism at its anterior end, and spread thence tailward. As a result of this, individual flexures progress cephalocaudad. (From G. E. Coghill, 1929, *Anatomy and the problem of behaviour,* Macmillan Co., New York, by permission of The Macmillan Company.)

with the motor cells only. At the beginning of the early flexure stage, they establish their connections with the sensory cells, and a pathway is established whereby an impulse can pass from sensory

cell through commissural neurone to motor cell. Since the early central sensory path conducts headward, and since the first commissural cells to complete their connections are located anteriorly, the earliest contractions appear anteriorly; since the early central motor path conducts tailward, they progress in the cephalo-caudal direction. The coil differs from the early flexure principally by the increasing number of more posterior segments added to those involved in the contraction The development of the S-reaction and of swimming is accompanied by the development of collaterals from the motor cells, the details of which need not be entered into here and now.

Having established the anatomical basis for the development of the swimming pattern, which represents one of the primary components of larval behavior, Coghill proceeded to analyze the development of two further aspects of behavior, namely the movements of the limbs, involved in terrestrial locomotion, and of the feeding reaction. On the functional side, he began by establishing first that the forelimb exhibits its first reactions (movements at the shoulder) only as accompaniments of trunk contraction; only later do shoulder movements occur independently of movements of the segmental musculature. Similar considerations hold true for movements of the gills and jaws, which occur first in conjunction with trunk movements and only later emancipate themselves into independent activities. Coghill succeeded in demonstrating the anatomical bases for these conditions, too: in the case of the early limb movements, the early concomitance of limb and trunk movements is based on the fact that the same motor cell branches to innervate both, and only later, when the movements become independent, have separate nerves arisen to innervate the limbs. The anatomical basis for the concomitance of gill and mouth movements with trunk movements is more complicated, related to the time at which specific central pathways arise.

It is relevant to repeat here the general interpretations which Coghill derived from these studies, namely, that "the behaviour pattern develops in a regular order of sequence of movements which is consistent with the order of development of the nervous system

and its parts" (1929, p. 36), and that "behavior develops from the beginning through the progressive expansion of a perfectly integrated total pattern and the individuation within it of partial patterns which acquire various degrees of discreteness" (1929, p 38).

Coghill's embryonic nervous system, like Sherrington's, is constructed of entities which resemble the latter's theoretical reflex-arc (where in the adult is any three-neurone pathway so clearly to be demonstrated as Coghill's *Rohon-Beard-cell-to-commissural-cell-to-motor-cell* pathway?), but in Coghill's nervous system, as in Sherrington's, the arc cannot act alone: in the amphibian the processes are present in the central nervous system which conduct forward toward another segment in the sensory tract, tailward toward another segment on the motor side, even before the internuncial neurones are completed which relate the one system to the other. Coghill's emphasis, like Sherrington's, is on the integrative aspects from the beginning, and not because of his own inclinations so much as by virtue of the facts of amphibian development made indubitable by his careful studies.

The clarity and incisiveness of Coghill's results on the amphibian material have led to the attempted repetition of his studies on mammalian embryos and fetuses Here unfortunately the results are less clear-cut, and controversies have arisen which will prove ultimately without great significance, as to whether or not Coghill's interpretations are applicable for transference to the available data concerning the genesis of the behavior pattern in the mammal

There are some investigators who believe that the mammal, like the amphibian, develops first a total pattern of behavior from which local reflexes later individuate, there are others who claim that in the mammal the local reflexes develop first, later to be compounded with others into a total pattern. The issue is confused by the fact that different investigators use different species for study, in which the sequence of events naturally varies (the mammalian embryo is notorious for its propensities toward telescoping developmental events, the biogenetic law notwithstanding), and by the fact that anoxemia, a condition difficult to eliminate in the handling of

mammalian material, has been postulated as preferentially masking the early local reflexes.

The most meticulous and accurate of the mammalian studies are those of Davenport Hooker on the human fetus (cf. Hooker, 1942, 1944). Hooker has shown that the first movement in the human elicitable by artificial stimulation is contralateral contraction of the upper trunk and neck musculature, accompanied probably by extension of both arms at the shoulder, in response to stimulation of the area innervated by the mandibular and maxillary divisions of the trigeminal nerve, a contraction involving, in reality, all the neuromuscular apparatus sufficiently developed to react. His results have been interpreted by Hooker, and probably with justice, to be consistent with the principles expressed by Coghill

It is true that the early total pattern here is less extensive than in the amphibian But in the human embryo, which has nowhere to swim in the amniotic fluid, early establishment of great longitudinal pathways comparable to those which govern the contraction of the segmental musculature responsible for larval swimming in the amphibian, would be utterly without biological significance, and it is not unnatural that it should lag in the mammal However the early development of the behavior pattern in man proceeds, the warning of Hooker must be respected, "that the development of early overt behavior in man is not the same as that in Amblystoma, nor is it the same as that in the cat, the rat, the rabbit or the sheep. *Each animal form exhibits a sequence in the development of its early overt behavior which is entirely its own*" [1] (Hooker, 1944, p. 33).

What emerges of importance from the developmental studies is not the particular sequence of events in any form, amphibian or mammal. Whether for one special reason or another, local reflex or general pattern seems for a particular form to be earliest expressed in overt behavior, it is indubitable that once the overt functional pattern of behavior has become established, the local reflex which is part of it is of importance primarily in its relationship to the integrated whole, and whether or not every vertebrate nervous system follows the specific pattern Coghill described for the amphibian,

[1] Italics by present author.

every embryo shows at some time long before its adult life is reached the subjugation of the segmental arc to a larger pattern, thus to attain the adult pattern of behavior. A more direct answer to the problem it is hardly fair to expect from embryology.

Functional Components in the Central and Peripheral Nervous System.—The emphasis here devoted to the integrative aspects of reflex action is not designed to minimize the great significance for neurological progress of the masterly analyses of reflexes begun by Sherrington and continued by his followers. It is because physiologists have learned to analyze experimentally artificially isolated reflexes and to work out their relationships to other reflexes that we have been able to fill in as much as we have of the details of our knowledge of central function. One outcome of our appreciation of the significance for somatic action of the manner of input into the central nervous system on the afferent side, and the manner of output from it on the efferent side, has been the accomplishment of a comparable analysis and synthesis for the autonomic system. Our recognition of the significance of specific receptivity allows us to add further particulars concerning the position of the specialized sense organs in the functional economy of the nervous system as a whole. The result has been the analysis of the whole nervous system in terms of so-called functional components. Such analysis of the central nervous system into its functional components, an analysis which has, by the way, been a preponderately American contribution (begun by Strong, 1892, 1895; Herrick, 1899; Johnston, 1901), is in large part responsible for our modern interpretations of central nervous action.

It has been realized since the days of Bell and Magendie that in the case of mammalian spinal nerves the dorsal roots are "sensory," the ventral roots "motor" in function. Since the dorsal roots may carry impulses that are never translated into consciousness it is desirable, and now conventional, to designate their fibers by the less prejudicial term *afferent;* the "motor" fibers may innervate effectors, such as glands, which do not "move," and accordingly they are best designated as *efferent.*

The reflex-arc as it was visualized by Marshall Hall (1833, 1843) as a result of the early experiments of Bell and Magendie and

their followers is one which seems to enable the organism to respond to a change in its external environment. The afferent nerve ending, or the sense organ interposed between it and the external world, is selectively excitable by some specific external environmental change, and has subsequently become known by Sherrington's term (1906) of *exteroceptor* The effector organ innervated is striated muscle, whose effective action is motion of the organism or of a part of it

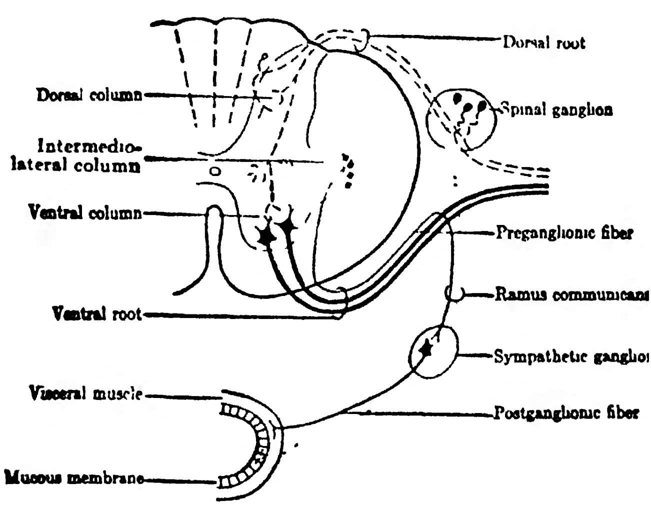


FIG. 29 Diagram illustrating the composition of a typical spinal nerve in the thoracic region The somatic sensory system is indicated by broken lines, the visceral sensory by dotted lines, the somatic efferent by heavy continuous lines, the visceral efferent by lighter continuous lines (From C J Herrick, *An Introduction to Neurology*, 1931, 5th edition, W B Saunders Co, Philadelphia, by permission of the author and W. B Saunders Co)

The afferent and efferent limbs of such an arc, concerned with the adjustment of the body to its external environment, have come therefore to be known as *somatic afferent* and *somatic efferent* components (Figure 29)

The autonomic nervous system has subsequently come to be recognized as analyzable into comparable components. The receptors in this division are called *interoceptors;* they respond to changes in the internal environment, especially in connection with the processes of respiration, digestion, excretion, and reproduction. The dorsal roots of the spinal nerves at some levels include *visceral afferent* com-

ponents, whose cell-bodies, like those of the somatic afferent neurones, are located in the spinal ganglia (Figure 2.9).

Visceral efferent components (Figure 2.9) innervate, in the case of the autonomic system, not striated musculature, but smooth muscle of the viscera, including the vascular organs, and the glands. The visceral efferent components differ from the somatic efferent neurones in some important respects. In the visceral system a two-neurone chain rather than a single neurone constitutes the efferent pathway from central nervous system to effector. The first neurones in the chain, the *preganglionic* neurones, have their cell bodies located more dorsally in the cord than those of the somatic motor system, in the intermediolateral rather than the ventral gray column. They terminate in the *sympathetic* or *parasympathetic ganglia* where they synapse with the *postganglionic* neurones which innervate the organs concerned. The visceral afferent and efferent components are responsible for the adaptive processes whereby the vertebrate maintains the constancy of its internal environment.

In association with the division of the autonomic system into the sympathetic and parasympathetic divisions, with the fact that sympathetic and parasympathetic fibers leave the central nervous system at different levels craniocaudally, and with the fact that not all of the autonomic ganglia are strictly segmental, not every spinal nerve includes all the components thus far described. What is important is the fact that some of them do This fact, together with the fact that particular portions of the embryonic cord give rise to specific components, has facilitated a similar analysis of the cranial nerves and the associated centers of the brain. It is this analysis which is responsible for much of our appreciation of the function of particular nervous pathways and centers.

The embryonic cord, on each side, consists of a dorsolateral *alar* and a ventrolateral *basal* plate, separated by a limiting groove (cf Arey, 1946): the dorsal part of the alar plate gives rise to elements concerned with somatic sensory, the ventral part of it to those concerned with visceral sensory activity. The dorsal and ventral portions of the basal plate are both motor in function, giving rise respectively to visceral and somatic motor columns.

A comparable pattern of development, with these four primary columns maintaining their relative positions, may be detected in the developing brain. The developing medulla oblongata shows especially clearly an arrangement comparable to that of the developing cord, and the higher levels of the brain may be comparably analyzed. Some investigators believe that diencephalon and telencephalon lack the basal plates, although both contain some elements directly related to motor control

Studies on the mammalian brain from such embryological considerations, together with a great body of comparative studies on the brains and cranial nerves of so-called lower vertebrates, where the overgrowth and specialization of the brain walls is often less elaborate than in the mammal, have made it possible to analyze the cranial nerves in terms of the same components as the spinal. The cranial nerves in some cases carry the kinds of components already described above, including afferent elements responsible for mediating responses to general sensation, touch, pressure, etc These elements, already described above, are called *general* components of spinal and cranial nerves alike. In addition, some of the cranial nerves carry special components; on the somatic side, those from the eye and ear, and on the visceral side, those from the organs of smell and taste are called *special somatic afferent* and *special visceral afferent* fibers (cf. Herrick, 1931) In addition, a small group of *special visceral efferent* fibers carried in a few cranial nerves innervate striated musculature, located primarily in the region of the jaws and pharynx, derived phylogenetically from the branchial arches of so-called lower vertebrates *Special somatic efferent* components innervate certain striated muscles of the eyeball and tongue highly specialized in origin.

Internal tracts and centers of the brain, whether primary, secondary or tertiary, can in many cases be related either directly or indirectly to one of these named functional components or to acting as intermediaries between them, and it is by virtue of such considerations that their arrangement has become intelligible The details of fiber-tract origins, connections, and relationships, which are not within our province to describe here, make up the substance of the

technical texts of neuroanatomy, to which the reader is referred for further details (cf. Herrick, 1931; Larsell, 1942; Ranson, 1947), with an admonition to keep in mind that it is only on the basis of integration of segmental and cranial afferent and efferent functions by the mechanisms already described, that the nervous system can act as a whole, and that without this in mind analysis of the functional components as of other isolated mechanisms must remain sterile for the development of any adequate concepts of the nature of nervous action

Sherrington (1906), who was responsible for the definitions of exteroceptors and interoceptors, described as *proprioceptors* the organs found in joints, muscles, and tendons (and the nerve endings located in the semicircular canals are now included in this category) which are stimulated by some body action or condition which is itself a primary reaction to excitation of an exteroceptor: these are the organs which are concerned in reflexes responsible for maintenance of tone, of posture, of equilibrium, of body attitudes, etc.

This group of receptors has long been considered as subsidiary in a way to the somatic group, and hence as related to the somatic system of functional components. In recent years, however, it has become apparent that end organs or nerve endings, *visceroceptors*, exist in the viscera and the vascular system which are "proprioceptive" in function, and the "proprioceptive" system is now considered as ancillary to the visceral as well as to the somatic system (Herrick, 1947, 1948). In addition, modern studies (Larsell, 1945) on the cerebellum, which Sherrington in 1906 (p. 347) in one of his brilliant analogies described as the "head ganglion of the proprioceptive system," have begun to reveal that this organ receives in mammals representation of a greater number of modalities of sense than had been anticipated, many organs of sense simultaneously performing both exteroceptive and proprioceptive functions, and have demonstrated the significance of this broadened input into the cerebellum for its integrative role in facilitating, regulating and refining the execution of all muscular activity.

Our old-fashioned conceptions of the "proprioceptive" system therefore stand in urgent need of revision It has been suggested

by Larsell and Herrick (Herrick, 1947, 1948), in view of the inadequacy of the current conception of the nature of the proprioceptive system and the faulty connotation of the term in present usage, that it might be well to avoid the term in favor of the more inclusive and appropriate name *proprius* system. The intermediate position of the proprius system, with its intimate relationships to both somatic and visceral systems, may prove, when more of the details of its interactions are known, to be one more factor of appreciable influence in integrating the activities of the central nervous system as a whole.

Interrelationships Between Neurones: Some Problems of Cortical Function

The Development of the Problem of Cortical Localization. It is the privilege of the investigator who deals with problems at other than the cortical level in many cases to be able to analyze the mechanisms with which he is concerned on a demonstrated basis of the relationship of structure to function. Such simple integrative mechanisms as those already discussed—the many possible connections of a single afferent neurone up and down and across the nervous system, the resultant convergence of many afferent endings on single neurones and on neurone pools and the consequent funnel action of the final common pathway, the varied connections and interrelationships of neurones known to be arranged in multiple chains and closed chains, the subjugation of the proprius system to both somatic and visceral systems—these are all examples of mechanisms which fit into this category.

The experimental physiologist can also point to other functional attributes of the central nervous system as a whole subserved by some of these same morphological constructions the multiplicity of connections described, together with the fact that transmission across the synapse is optional rather than obligatory, may account for much of the flexibility of action and reaction which is a distinguishing feature of the vertebrate central nervous system.

The investigator of cerebral action at the cortical level is not so fortunate. On the morphological side, he is faced with the recur-

rence of variations on a pattern which repeats over many areas of the cortex; he cannot be certain as yet either of the significance of the pattern itself, or of the meaning of its local modifications. On the functional side, he is gradually developing confused awareness of the fact that another great pattern exists; but even when he resists the temptation to consider problems of emotional and intellectual behavior and confines his attention to the strictly motor and sensory, the details of the pattern still escape him. To make matters more complicated still, he does not yet know surely even what functions are controlled in the cortex: it is only since the days of Head (Head and Holmes, 1911) that it has been firmly established that some influence over consciousness is exerted elsewhere. It is small wonder, then, in the face of his uncertainties both on the structural and functional sides, that he is unable to "explain" the function of the cortex by its structure, and to demonstrate the relative roles of structure and function in producing the integrated and integrating action of the cortex. What is remarkable is that, in the face of his limited data, he can have made as much progress as he has.

Suspicions that different parts of the brain perform different functions may be very ancient: the Hippocratic treatise on head injuries noted that wounds of the temporal region on one side might result in convulsions on the opposite side of the body, and spotty observations of the sort characterize many writings between the ancient and modern periods. As late, compared to Hippocrates, and as early, compared to our own time, as 1691, Robert Boyle recorded observations concerning a knight who suffered sensory and motor paralysis ("dead palsy") of arm and leg following a depressed fracture of the skull, whose symptoms disappeared after the surgical removal of a spicule of bone pressing on the dura (Fulton, 1943). Willis, in the seventeenth century, finally established to his own satisfaction the cortex as the seat of ideas and of memory; Swedenborg, in the eighteenth, an anatomist before he turned mystic, localized all of the functions of the soul in the cortex, and even ascribed to its *corpuscula* (what we know now as the pyramid-cells) a part in the function of translating sensation into motion. These early and rela-

tively isolated observations and speculations, with many others, led to the elaboration of no over-all inclusive theory; but they may have helped to prepare the scene at least for the prologue to the modern drama.

It was Gall, probably partly under the influence of Lavater who was setting the fashion for physiognomy, who finally expressed the *doctrine of cerebral localization* Gall's emphasis on dissection, rather than section, as a method of investigation of the brain, was strong, and he was specifically interested in dissecting fibers and what we would call fiber-tracts, which he followed in many cases as he thought to the cortex He concluded that "the convolutions of the brain are nothing but the peripheral expansion of the fascicles of which it is composed; consequently, the convolutions of the brain must be recognized as the parts where the instincts, sentiments, penchants, talents and, in general, the moral and intellectual forces are exercised" (quoted from Temkin, 1947, p 279)

Gall's misconceptions, in retrospect, were two In the first place, while he considered the cortex the organ of intelligence, he proceeded to localize within it not only organs responsible for the control of intellectual but also those concerned with moral qualities, good and bad Secondly, he considered the bony cranium to reflect externally the development of the organs localized in the cortex, and his followers unfortunately chose this least fortunate of his notions to exploit Spurzheim, in particular, laid his emphasis on Gall's theory that irregularities on the surface of the skull were related to the configuration of the brain below, which in turn was related to the development of particular mental functions and talents. Gall himself had minimized the significance of the skull "The object of my researches is the brain The cranium is only a faithful cast of the external surface of the brain, and is, consequently, but a minor part of the principal object" (quoted from Temkin, 1947, pp 277-278). But the damage had been done, and his theory was perverted into the disreputable but overwhelmingly popular pseudoscience of phrenology. Yet naive and discredited though the phrenologists may have been, they had their influence too and may well have served to open the way toward acceptance of the more modern doctrines once these were enunciated.

Flourens (1823) attempted early to contravert the doctrines of functional localization on the basis of his experimental studies on various animals. Actually, however, although his own emphasis was always on the importance of the integrity of the cortex, his own experiments, performed in 1823, first established on a sound basis the dependency of a specific function, in this case vision, on cortical integrity. He demonstrated experimentally that the eye seemed blind on one side after ablation of one hemisphere, and that complete blindness followed removal of both hemispheres. Panizza localized visual function more accurately, as early as 1855. Having studied at autopsy the brains of two patients, one of whom was blind following apoplexy, the other following unilateral optic atrophy, he first concluded the parieto-occipital cortex to be essential for vision. As a result of his consequent experimental ablations on dogs, he finally localized visual function in the more circumscribed area of the occipital cortex. His work, however, was soon forgotten, and further prosecution of comparable studies had to await a new awakening of a new generation.

If Gall divined the fundamental problem of cerebral localization, it remained for Broca finally to pose it as we know it and to initiate the modern studies which have attempted to answer it. Broca, in 1861, passed far beyond his predecessors, beginning with Gall,[2] who had localized the faculty of language in the anterior part of the brain, by localizing a center for "articulated language" in the posterior portion of the left third frontal convolution, on the basis of observations on "aphemics" (Broca defined *aphemia* as the inability to pronounce words in the absence of paralysis of the tongue) in whose brains circumscribed lesions were evident at autopsy.

The spectacular and unexpected demonstration that change in function of as highly complex a process as the use of language could be correlated with visible destruction of a discrete and particular area of the cortex gave rise immediately to a new and changed conception of functional localization in the cortex. It took only a few years for

[2] Riese (1947) calls attention to the fact that the first accurate description including the basic features of aphasia was made for the case of an apoplectic described by Goethe, in *Wilhelm Meisters Lehrjahre*, who associated the condition with paralysis of the right side, thus with a brain lesion on the left. Goethe's maternal grandfather had been afflicted with right hemiplegia accompanied by disturbances of speech.

Hughlings Jackson to start issuing his felicitous warnings; that "speechlessness does not imply wordlessness" and that "to locate the damage which destroys speech and to locate speech are two different things" (*1874; quoted from 1932, II, pp. 131, 130*), that it is not the part destroyed, but the tissues remaining after destruction, which are ultimately responsible for change in function—all admonitions which have been too often ignored even by investigators working on far simpler functions, to the detriment of our modern conceptions. Whether it is fortunate or unfortunate that localization of one of the most complex of intellectual functions was first to be studied exhaustively, and whether or not the first studies were accurate or correctly interpreted, the fact remains that they awakened a new interest in the problems of localization, and stimulated the progress of the great body of investigations which followed.

Complex though the faculty of language use may be, and complicated in its relationship to the problem of localization, Hughlings Jackson was immediately able to draw from Broca's early reports an inference that a simpler problem existed. Only five years after the publication of Broca's first cases, he wrote: "There is no more difficulty in supposing that there are convolutions . . . for superintending those delicate movements of the hands which are under the immediate control of the mind than that there is one, as Broca suggests, for movements of the tongue in purely mental operations" (1866; quoted from 1932, II, p. 123). This was really a new concept of localization, visualizing, as it did, the possibility of localized representation in the cortex of movements of specific groups of somatic muscles. Hughlings Jackson went further and suggested, on the basis of what is now known in his honor as the "Jacksonian march" in "focal" epilepsy, that these local areas of cortical representation are organized in some kind of orderly arrangement, a fact which was independently and almost immediately confirmed by Fritsch and Hitzig's (1870) demonstration that electrical stimulation of the frontal cortex in the dog and other mammals resulted in movements of the extremity of the opposite side. In 1874 Hitzig published his monograph delimiting more accurately the motor area in dog and monkey, and already, the year before, Ferrier (1873) had demonstrated that removal of the "hand area" in the monkey resulted in

paralysis of the opposite hand, thus effecting a fusion of clinical with experimental investigation that was destined to play a great role in accelerating neurological progress.

The human cortex was stimulated as early as 1874 by Bartholow, who reported that stimulation of the postcentral convolution, exposed through a skull wound, gave rise to sensations referred to the legs. Other occasional reports of the kind followed, but clinical and scientific investigations of the brain awaited the genius of Harvey Cushing, and it was primarily as a result of his imagination and courage that in 1909 the way became clear for systematic exploration of the human cortex, and that a large body of accurate physiological data became available whereby the actions and reactions of the human brain itself could be exhaustively studied.

Once the existence has been established of a cortical pattern of representation, the problem arises as to its organization, its structural foundations, its functional operations, the nature of its control over processes occurring at "lower" levels of functional differentiation (again a wise concept evolved early by Hughlings Jackson), the character of its own basic mechanisms of integration. The remainder of this chapter will attempt to review briefly some of the results of some of the investigations which have begun attempts to deal with some of these problems.

Structural Localization in the Cerebral Cortex: Architectonics.—It has been known at least since 1776, when Gennari observed a white line, now known as the line of Gennari (Figure 2.10), within the cerebral cortex and especially marked in the calcarine cortex of the occipital lobe, that the cortex has a special structure of its own (Gennari, 1782). The science of *architectonics*, however, which attempts to subdivide the cortex into regions of specific structure, is largely a creation of the present century. Its development may have been an inevitable outcome of the vast number of physiological studies of the cortex which followed the work of Fritsch and Hitzig: von Monakow, in 1902, could present a bibliography containing 846 entries referring to the localization of somatic function in the precentral cortex alone; the materialism that dominated science at the turn of the century may well have led investigators to turn to

morphological studies for an answer to problems apparently otherwise insoluble

Flechsig (1876), realizing that fiber-tracts become myelinated at different periods of development, and laboring under the belief (now known to be mistaken, cf Angulo y Gonzales, 1929) that they begin to function only when their myelinization is complete, attempted parcellation of the cortex into some 45 areas on the basis

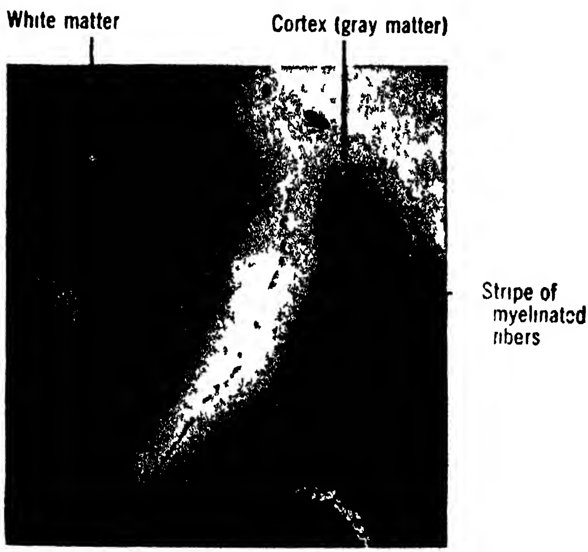


FIG 2 10 Photomicrograph of a section of occipital cortex The entering visual fibers form a band or stripe as they end in the fourth cell layer Weigert stain (From E Gardner, *Fundamentals of Neurology*, 1947 W B Saunders Co, Philadelphia, by permission of the author and W B Saunders Co)

of the chronology of myelinization of fiber-tracts related to particular cortical areas The more influential studies, which followed in the early part of the present century, used primarily a combination of two methods to subdivide the cortex, namely, the study of cell types (*cytoarchitectonics*) and the study of myelinated fibers (*myeloarchitectonics*). The cortex is often considered as constituted, though Lorente de Nó (1943) has questioned the validity of the concept, of six primary layers of cells and fibers, varying from area to area in absolute thickness of cortex, in absolute thickness, relative thickness,

number and arrangement of layers, and in type, shape, density, relative and absolute size, staining affinities of cells, and so forth (Figure 2.11). On the basis of such criteria, the human cortex has been

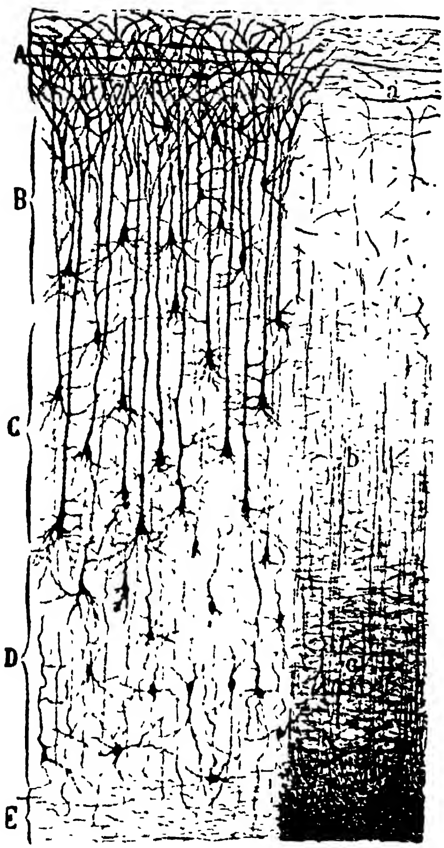


FIG. 2.11. Section of the cerebral cortex of a rabbit of eight days. The right portion of the figure represents the medullated nerve fibers, revealed by Weigert's method. The left portion shows the nerve cells, as they appear with the method of Golgi. A, molecular layer; B, layer of small pyramidal cells; C, layer of large pyramidal cells; D, layer of polymorphous cells; E, white substance; *a*, medullated fibers of the molecular layer; *b*, bundles of axis cylinders; *c*, plexus of collaterals. (From S. Ramón y Cajal, *Histology*, revised, authorized translation from 10th Spanish edition, 1933, William Wood and Co., Baltimore; by permission of the publishers.)

divided by Brodmann (1925) into over 50 distinctive areas, by von Economo and Koskinas (1925) into roughly twice as many, and by Rose (1936) and others into as many as 200.

Similar studies have been made on the brains of other mammals, and areas comparable to those of the human cortex have been distinguished in some cases In an attempt to standardize the architectonic areas of the brain of the spider-monkey, Lashley and Clark (1946) have recently found that individual variations in cell size, density, and arrangement exceed many interareal differences, rendering thoroughly unreliable the criteria upon which earlier parcellations have been based; they have found only seven large areas which they can consistently identify, and they find subdivisions of these to be based only on variable characters which they presume to lack functional significance They believe that "marked local variations in cell size and density among individuals of the same species may constitute a basis for individual difference in behavior" (1946, p 300), a thesis which will take a great deal of work to confirm; but their critique of architectonics has done great service in revealing the fallacies of the attempts to over-divide the cortex on a morphological basis, and once more returns the burden of analyzing patterns of cortical action to those concerned primarily with its function The existence, however, of the condition of *cerebral dominance*—the usual preponderance of motor control over speech in the left hemisphere of right-handed individuals known since the days of Broca, the absence of marked morphological difference between left and right hemispheres—these facts should have presented sufficient evidence that the essential features of the localization pattern are functional rather than structural in nature.

Functional Organization of the Cerebral Cortex.—Hughlings Jackson's original surmise that there is some orderly arrangement in the connections of parts of the cortex, which results in an organized pattern of cortical control of overt activity, has been corroborated by a great mass of data accumulated from the study of clinical and experimental material, and it remains to review briefly the results of some of the studies which demonstrate the existence and the character of such a pattern.

Before proceeding to a discussion of the results, however, it may be relevant to comment on some of the methods by which they

have been procured, since some of them require great caution in interpretation.

Deletion or extirpation experiments are the best that surgeons can make at the moment, but their contribution to the theoretical worker does not equal the benefit received by the patient, because of the difficulties inherent in the interpretation of the outcome. No deletion experiment on living material can ever demonstrate with absolute certainty the function of the part removed, because of the great power of regulation inherent in the organism; what remains after the deletion operates in such a way as to compensate for the function of what has been taken away. This is as true for the cortex as for other biological material. Lashley (1929) has claimed for the rat that the capacity to learn a maze after cortical destruction is related rather to the amount of tissue destroyed than to the particular area deleted (*Law of mass action*). This generalization is not applicable to man At least one patient, however, has been described who not only survived complete removal of the right cerebral hemisphere, but who was even able to walk without support from a crutch (cf. Fulton, 1946); patients in whom the corpus callosum has been divided throughout its extent show a delay in response time which seems to be the only well-established disturbance resulting from severing the paths which connect the two hemispheres with one another (Smith, 1947).

The method of artificial stimulation is fortunately safe for clinical use, and especially valuable to the experimental investigator in that it gives positive results and can be quantitatively controlled Even so, as will shortly become evident, the results of its application are not always reproducible. This is bound up with the properties of what is being stimulated, rather than caused by any defect in the method, therefore itself of value to recognize; but it points to the necessity of developing additional new methods of attack.

At present new methods are being developed which are exploiting the electrical properties of the brain. It is too early to appreciate the significance of the Berger rhythms which emanate from the resting brain, or even to know to what extent they are cortical in origin, but the study of local changes in potential is becoming fruitful (see Jasper, 1948, for recent key bibliography).

Local application of strychnine to the cortex, which has been known for over thirty years (Dusser de Barenne, 1916) to give rise to sensory phenomena (hyperaesthesia, paraesthesia, hyperalgesia), is now being used in conjunction with electrical study of the brain. It is now known that such local applications may be followed by bursts of electrical activity in parts of the cortex or of the thalamus related to the area stimulated Strychnine is a powerful drug, and this is a drastic and acute experiment, open to many criticisms, but it performs a useful service in demonstrating interrelationships of parts, and reveals the possibilities for more refined comparable techniques; already in some cases peripheral stimulation has been shown to be reflected in localizable electrical activity of the cortex (see pp 101 ff below)

One difficulty with all experiments in which the brain surface is exposed is that it has been found expedient to stimulate or study the cortex of patients only under local anaesthesia, it is difficult if not impossible to handle animals in such experiments except under general anaesthesia One of the facts of which we can be most certain, so far as cerebral function is concerned is that it may be altered by anaesthetic drugs Results obtained under differing conditions of anaesthesia are rarely comparable, and hence many data obtained from animal experimentation are not applicable to man Far more important than this, the brains themselves differ in very marked degree. Removal of the calcarine cortex in man (Marquis, 1934) is followed by loss of object vision and loss of light perception, in the monkey (Marquis and Hilgard, 1937), loss of object vision results from bilateral removal of the occipital lobes, but primitive light discrimination of a crude type persists, the dog can discriminate between differences in light intensity, after bilateral removal of the occipital cortex, almost as well as before (Marquis, 1934). Rats, after the removal of the visual area, lose pattern vision but retain sufficient power to appreciate differences in light intensity to enable them to avoid obstacles and to recognize food by sight (Lashley, 1931, 1941). No comparison could better warn of the danger of applying results obtained on the cortex of one animal form to interpretations of that of another.

Despite the limitations, however, of some of the methods of attack that have been employed, on the whole great progress has been made. In the first place, Hughlings Jackson's conviction that "the whole of the anterior lobe is (chiefly) motor" and "that the posterior part of the brain is (chiefly) sensory" (1887; quoted from 1932, II, 79), has been reasonably well justified by the studies subsequent to his own, in spite of the fact that there is some overlap as he suspected.

Stimulation of the human cortex has shown the existence of five principal *projection areas* with known direct fiber-tract connections to other parts of the central nervous system (Figure 2.12). The

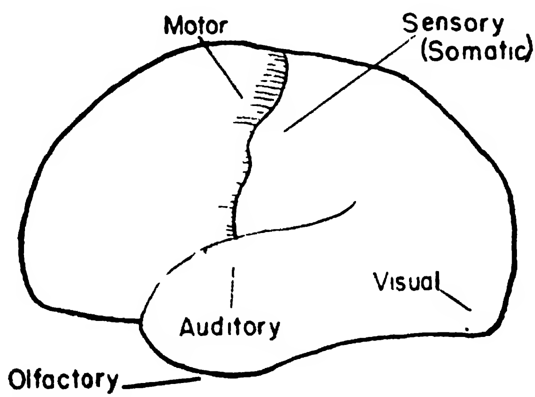


FIG. 2.12. Position of the five "projection areas" in the human brain. (After E. D. Adrian, *The Physical Background of Perception*, 1947, Clarendon Press, Oxford, by permission of the author and the Clarendon Press.)

projection area of the cortex for vision is located in the calcarine region of the occipital lobe, that for auditory function in the transverse gyrus of Heschl in the temporal lobe, that for somaesthetic sensibility in the postcentral convolution. The precentral convolution represents the projection area exerting control over somatic and in some cases visceral motor activity—Hughlings Jackson had concluded in 1875, with his customary perspicuity, that visceral function has extensive cortical representation. Interestingly enough, each of these areas is included among the seven which Lashley and Clark (1946) were able to identify on architectonic grounds.

The areas adjacent to these, the "silent areas," have been thought of as lacking the direct subcortical connections which have been

supposed to characterize the primary projection areas; it is now becoming apparent that the thalamus "projects" to them as well as to the others in some cases, and some revision in our thinking may be required (Walker, 1938). In any case, these areas are less excitable by direct stimulation and are believed, on the basis of clinical and experimental studies, to be association areas concerned with functions related to those in bordering projection areas. Stimulation, for instance, of the auditory cortex gives rise to sensations of the nature of ringing in the ears, buzzes, clicks, roars, and so forth—never words; in contrast, tumors in areas of the temporal cortex bordering on the gyri of Heschl may give rise to auditory hallucinations in which speaking voices and beautiful music are "heard"; similarly, stimulation of the calcarine cortex in man may give rise to sensation of light; tumors of the occipital area bordering on the visual cortex may result in visual hallucinations (Penfield and Erickson, 1941). Partly as a result of the apraxias which follow lesions in the parietal cortex posterior to the postcentral convolution, this area is currently believed to be of great significance in the synthesis of formulated patterns of skilled activity, patterns translated somehow, somewhere else, into useful adaptive movement.

It is patently impossible, in a review of the present limited scope and dimensions, to include more than a bare fraction of the available details from modern studies concerning cortical action. Since the projection areas represented by the postcentral and the precentral gyri have been exhaustively studied, and since some of the problems they present are relatively simple, at least in contrast to those raised by the association areas, further discussion will be confined primarily to considerations of these two areas.

It is clear, from electrical stimulation of the human cortex, that there is an orderly topographic representation of movements of body parts in the precentral convolution, with the lower extremity of the opposite side represented dorsally in the convolution, the upper extremity below it, and the cranial muscles most ventrally; the homunculus of Penfield and Boldrey (1937) represents graphically and vividly the order, and relative magnitude of extent, of the representation of various parts on the convolution (Figure 2.13). The comparatively large size, in the diagram, of the area for thumb, lips,

and tongue draws attention to the fact that these parts occupy long segments of the cortex as measured in individual cases, in keeping with the utilization of these parts in speech and in the highly skilled movements of the hands. Control of skilled movements is a primary function of the motor area.

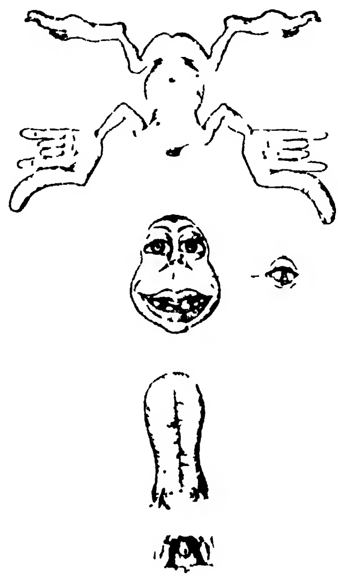


FIG. 2.13. Sensory and motor homunculus. This was prepared as a visualization of the order and comparative size of the parts of the body as they appear from above down upon the Rolandic cortex. The larynx represents vocalization, the pharynx swallowing. The comparatively large size of the thumb, lips and tongue indicate that these members occupy comparatively long vertical segments of the Rolandic cortex as shown by measurements in individual cases. (From W. Penfield and E. Boldrey, *Brain*, 1937, 60, 389-443; by permission of Dr. Penfield and the Editors of Brain.)

Granted that there is a serial representation in the cortex of motor functions of various parts of the body, it remains to investigate the nature of the control over motor activity which is exerted by the cortex. Clearly we need to know more than that artificial stimulation of the cortex in the motor area gives rise to movements of the parts concerned.

One fact, reasonably well established, seems to be that the giant pyramidal cells of Betz, the cells that give rise to the fibers con-

stituting the great corticospinal pathways, which impinge directly on the motor horn cells of the cord, are the primary cells involved in strictly motor cortical action. These cells are found in the fifth layer of the motor cortex. When the upper four layers of the cortex are destroyed by coagulation with heat, a drastic experiment, the excitability of the area is unchanged; when all six layers are destroyed, it is completely abolished (Dusser de Barenne and Zimmerman, 1935). It is likewise abolished by transection of the pyramidal tracts (cf. Tower, 1940, 1944, for modern discussion).

There are many properties of the area on which opinion still varies. Extirpation of the motor cortex, in whole or in part, has been widely practiced since Ferrier's first experiment in 1873; yet even now investigators are at odds as to whether the resultant paralysis is of spastic or flaccid type. In some cases paralysis may be flaccid for proximal and spastic for distal joints in the same limb. The results differ when this area is removed alone, and when adjacent areas are also involved; the time of removal of the premotor cortex is a factor varying the results of motor area extirpation. There is disagreement, also, as to the possibility of ipsilateral as well as contralateral representation (cf Fulton, 1943, and Bucy, 1944b, for discussion of these problems)

Particular cortical points, moreover, may be inconveniently, from the point of view of the investigator, variable and unstable when artificially stimulated: in some cases stimulation of a particular point may elicit different movements at different times, i.e., response from a single point may be reversed from flexion to extension of a limb (Brown and Sherrington, 1912). Stimulation of one point may result in a change in the action evoked by stimulation of an adjacent point; in the human, a motor (or sensory) point has been displaced to a distance of as much as 4 centimeters by repeated advancing stimulation of previously unresponsive territory (Penfield and Boldrey, 1937, 1939) Stimulation of one point may affect the results of stimulation of another in some cases by facilitation, in others by extinction. There is an electrically excitable suppressor band, the *strip area* (Hines, 1936, 1937), anterior to the motor area, whose stimulation results not only in inhibition of contraction on the opposite side, but also in a rise in threshold of the motor area

and in suppression of its spontaneous electrical responses (Dusser de Barenne and McCulloch, 1939).

There is little doubt but that understanding of motor control by the cortex will grow in measure as the analytical studies improve in technical perfection. It is only within a year that the first reports have appeared which have dealt with the effects of cortical stimulation myographically, in terms of contractions of isolated muscles, rather than as parts of whole limbs observed through the skin (Chang, Ruch and Ward, 1947); it is clear, as a result, that the Betz cells innervating a given muscle, in the monkey, are concentrated in fields, each with a central focus and a peripheral fringe. For individual muscles, as for larger functions, there is rather focalization than localization of representation in the cortex. This study raises more questions than it answers, but it is significant in its emphasis on field patterns rather than on fixed points.

The precentral cortex, to borrow an analogy from a contemporary clinician, has been likened to a "motor keyboard, and useful movement is its music. . . . The true function of the keyboard can only be demonstrated when it is played upon by the ordered impulses that arise in other and 'higher' neuronal circuits" (Penfield and Erickson, 1941, pp. 48-49).

The impulses that emanate from the other "higher" circuits, whatever and wherever they are, must be directly or indirectly related to the afferent input into the central nervous system, and this remains to be discussed at what is presumably its highest level in the sensory projection areas.

The topographical order of representation of body parts is much the same in the somaesthetic area as in the motor. In the case of this area, furthermore, it has been possible, by stimulating the skin and localizing the site of electrical activity on the cortex, to project the body surface on the postcentral gyrus in great detail (Figure 2.14), and it is interesting that a dermatomal organization occurs in the cortex comparable and presumably related to the peripheral pattern of dermatomes, overlap and all (Woolsey, Marshall and Bard, 1942). Here again, as in the motor area, the hand projects to a large area of cortex, as does the face: Adrian postulates, as an explanation of the latter phenomenon, the fact that man is descended

from animals without hands which use snout and long vibrissae of the face as the most sensitive tactile organs, and supports his theory by calling attention (1943, 1946) to the relatively large receiving areas in the cortex for the pony's nostril and the pig's snout.

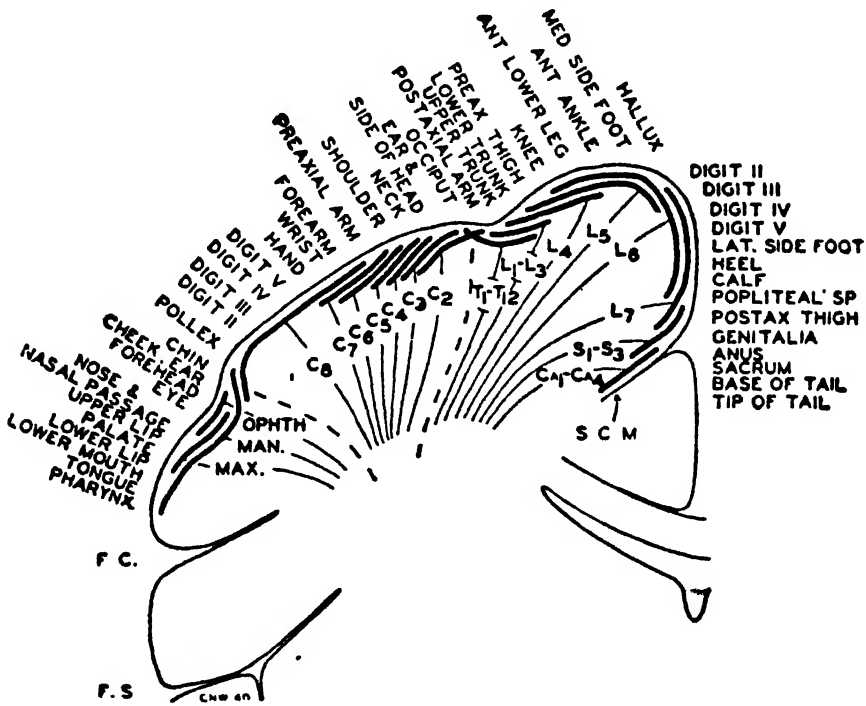


FIG 2 14 Schematic frontal section through postcentral gyrus of monkey illustrating the dermatomal projection and overlap of successive dermatomes of the parietal lobe. Note that whereas the sacral, lumbar and thoracic dermatomes proceed in orderly sequence, the order of the cervical dermatomes is reversed F S. Sylvian fissure; F. C, central fissure, S C M, callosomarginal sulcus, MAX, maxillary, MAN., mandibular, OPHTH, ophthalmic, C, cervical, T, thoracic, L, lumbar; S, sacral; CA, caudal (From C. N Woolsey, W H Marshall and P Bard, *Bull. Johns Hopk. Hosp*, 1942, 70, 399-441, by permission of Dr. Woolsey and the Editors of the Bulletin of the Johns Hopkins Hospital)

Any comfort that neurologists may have derived from the apparent simplicity of plan which provides a certain degree of correspondence between the position of comparable points on precentral and postcentral convolutions has been dissipated recently according to the usual fate of generalizations It has recently been shown, for

lower mammals, that there is another area, below and behind the primary one, receiving impulses as a result of somaesthetic stimulation and in some cases this accessory area receives from both sides of the body (Adrian, 1941). Something analogous has been demonstrated for the auditory cortex in the dog (Figure 2.15); there is a primary strip whose anterior regions are active in response to notes of high frequency, whose posterior regions are active in response to notes of low frequency. In addition there are two secondary strips below, one anteriorly which this time is related to sounds of

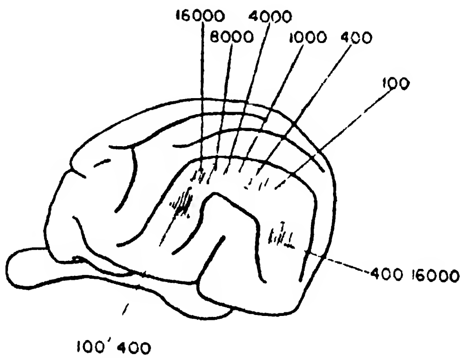


FIG. 2.15 Auditory area in the dog's brain The figures indicate the frequency of the sounds which produce the maximum potential change in different parts of the area. There is no corresponding localization on the lower two areas; the anterior reacts to all notes between 100 and 400 cycles and the posterior to all between 400 and 16,000. After A. R. Tunturi, *Am. J. Physiol.*, 1944, 141, 397-403; by permission of the author and the Editors of the American Journal of Physiology.)

low frequency, the reverse for the posterior. These two separate smaller strips are stimulated only in the case of loud sounds; a loud sound thus produces activity in two cortical regions (Tunturi, 1944). The meaning of these secondary areas is not yet clear, but there is evidence at least that our old notions of sensory localizations stand in urgent need of revision.

We have been speaking here primarily in terms of motor and somaesthetic control, since this has seemed easiest to attack experimentally. Although great progress has been made in demonstrating mechanisms of cortical action, tremendous problems still remain to be analyzed, even for simple motor and somaesthetic action. Where the next advances will come is hard to prophesy; study of the cere-

bellum and thalamus is at the moment in ascendency, and perhaps much will be learned about the cerebral cortex in relationship to these. New clues to motor and sensory activity may well come from a study of the relationships of the cortex to subcortical structures or to other cortical areas.

The synthetic and integrative properties of the cortex, which cannot be entered into here, will demand analysis at a different level, certainly, than the somaesthetic and the motor. What the integrating operations are, we do not know. Whatever the integrative agencies are, however, we do know that they do not work through "a disquieting figure of a little hobgoblin sitting up aloft in the cerebral hemispheres with a series of maps to look at," to use a metaphor of Adrian's (1947, p. 48) out of its context. For somaesthetic and motor action, pattern rather than point is of transcendent importance.

Most of what little we can now infer about cortical function can be looked at either negatively or positively. We can say that localization is not so strict as was once believed; we can add that there is a pattern, which may challenge analysis but which at least is there to analyze. We can say that whatever pattern exists has a less strictly structural basis than once was hoped, but we can comfort ourselves that this is no problem special to the cerebral cortex. It may be even more difficult to deal with in the case of the diffuse neuropil, or for the cerebellum where the cortex exhibits far greater uniformity of structure. By being able to exclude structure alone as the basis for differences in action, we make one great advance: we know at least that it is with the analysis of the functional pattern that we must proceed toward the ultimate solution.

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CHAPTER 3

SOME NEUROLOGICAL CORRELATES OF BEHAVIOR

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Introduction

The facts of personal experience, of being aware, are our most immediate certainties. The essence of our conscious life--warmth and cold, tones, colors--is a class of facts requiring no inferences. And the search for the bodily correlates of our experiences has led through many avenues of exploration. One certain concomitant is to be found in the nervous system. A study of the neurological correlates of "behavior-experience" (Yerkes, 1941), however, is so broad that, for the present purpose, it must be narrowed. Our primary interest perhaps can be indicated by reference to some of the areas which will be disregarded.

A description of the physiology of the central nervous system alone is barely condensed into a single section (Chap. 2), and that report necessarily excludes a detailed account of reflexive behavior (cf. Sherrington, 1906 and 1947; also, Fearing, 1930). Psychosomatics, as another illustration, is concerned with the emotional factors in the disturbance of various vegetative functions. Dressed up with a new name and considerable significant research, this important area of psychology nonetheless "deals with one of the oldest, if not *the* oldest, problems of scientific thought—with the mind-body problem" (Alexander, *et al.*, 1948, p. 3).

The delimitation of the present area of interest is, of course, arbitrary, and a precise formulation is impractical. Generally speaking, our concern will be with the bodily correlates of the integrative behavior manifested in such processes as learning, perceiving, and thinking. Throughout, the emphasis will be upon the evaluation

of the more important problems, the experimental technics and the possible future developments.

The questions that one can raise would comprise a formidable list. Is there a specific cerebral locus for the conditioned reflex? Is the response itself an essential component in conditioning? What is the cerebral contribution in maze learning; are there specific neural engrams, or does the brain "act as a whole"? What is "retained" in the nervous system as a consequence of learning? What is the neural representation for relational processes—form discriminations, the experience of triangularity, or squareness?

There is another way in which the problems of cerebral dynamics can be posed. Are the *inter*neuron relationships organized responses of nerve impulses, akin to those of the telephone dialing system which works by a succession of equal, discrete, electrical impulses grouped in a temporal pattern? This approach, in one form or another, represents the "molecular," or "particle" description, based upon the patterning of discrete impulses in different neurones. This view is represented by traditional neurophysiology. To take an opposed view, are the cortical mechanisms *molar* field forces? Are the patterns of excitation fundamentally *field* functions literally akin to the physical lines of force demonstrated by soft iron filings on paper activated from below by a magnet? The molecular, and historically older, view is demonstrated in the history of the reflex arc, in the *assumption* of reduced synaptic resistance in learning, in the physiology of the nerve impulse, and in the several varieties of behavioristic theory. The molar approach was initially made by the Gestalt psychologists in emphasizing the organized dynamics of perceptual processes and the implications of physical field concepts for psychology.

Definitive answers to *none* of the above questions are as yet available. However, there has been considerable progress in neurophysiology. Equally important is a somewhat better understanding of the nature of psychological activities. For these reasons, our questions can be formulated more clearly than heretofore. Some, if not all, of them may have been either improperly put, or it might be possible to reduce them to more fundamental propositions. The basic questions could be those of *equivalence* and *retention* (Adrian,

1947). But even this formulation may prove to be inadequate; often the real question is not appreciated until the answer has been discovered.

Since final answers are still not possible, the attempt will be made to supply what is known about the several problems listed above in question form. Attention will be centered largely on the problems of cerebral dynamics, although there will be some consideration of other mechanisms. It is assumed by many, and may very well be true—conclusive evidence being lacking— that the key integrative mechanism in man is the cerebral cortex. The complexity of the apparatus to be examined can be readily appreciated. The number of neurons in the human cerebral cortex alone has been estimated to be in excess of 9,000 million (9×10^9). It is doubtful that the normal functional interneuronic relationship is ever limited to two at a time, although such may be obtained in special researches (cf. Shurrager and Shurrager, 1946). It is more likely that thousands of cells fire in all sorts of possible relationships. To this should be added the reverberatory, "circular," circuits reported by Lorente de Nó (1938). And, finally, in addition to the well known one-way, "all or none" transmission, there may be spread of continuous excitation (Lorente de Nó, 1934). Clearly, the potential neural relations are many. And theoreticians have taken advantage of this fact as demonstrated by the different hypotheses which have been offered to explain cerebral functions. The cerebral apparatus would appear to be sufficiently complex so as to be able to satisfy any theory devised to explain its role in behavior.

A proper perspective is furthered by an appreciation of the methods by which the data were procured. One must be extremely careful in extrapolating from animal to man. "Men are bigger and better than rats" (Herrick, 1926, p. 365) and introspections are lacking in animals. The limitations of clinical data are equally important and will be discussed specifically in the appropriate place. There is another factor, largely ignored or merely given verbal allegiance. The role of hereditary and genetic experiential determinants, in both infra and human species, is not well understood. As a general methodological consideration, therefore, *operationalism* rightly emphasizes that data are no better than the technics and conditions

under which the observations are obtained (Stevens, 1935; and Boring, *et al*, 1945).

"Simple" Processes

Conditioning.—From the vast array of available data, the complexities of the problems to be faced may perhaps be best introduced by considering those which stem from reflex theory. The theory is most attractive because of its simplicity, although it can be anticipated that this apparent advantage will be only superficial.

If heredity makes available a host of simple reflex-units, then one can readily conceive of the complex behavior patterns arising from a concatenation of these "simple" reflexes. "Association of ideas" is thus represented objectively by *conditioned* reflexes, in which an initially neutral or indifferent stimulus comes to evoke the reflexive activity (cf. Watson, 1916).

Experimentally, the animal is given repeated paired trials of the neutral or *conditioned stimulus* (CS), such as a bell, followed by the natural or *unconditioned stimulus* (US), food or electric shock. At first, the salivation (or limb flexion) occurs as the unconditioned response (UR) to the US, food (or electric shock). After some trials, salivation (or limb flexion) occurs as a *conditioned reflex* (CR) to the CS alone

If each stimulus arouses a specific neural engram, then the conditioning process consists of a synaptic "switching" from one pathway to the other. As we shall see, however, not only is the neurological mechanism quite complicated, but the behavior itself is not at all simple Most of the earlier workers believed that the learning was determined by the cortical mechanism. Pavlov's theories, for example, were firmly based upon the assumption that the cerebral cortex was essential for conditioning (Pavlov, 1927, p. 330).

This description must be modified, however, if the cortex is dispensable, a view enunciated by Herrick (1926, p. 167) years before the actual experimental disproof of Pavlov's dogma was made. Today, it is unequivocally established that "learning" can occur in totally decorticated mammals (Culler and Mettler, 1934; Girden *et al.*, 1936; Ten Cate, 1934; and Bromiley, 1948a). The implica-

tions of this conclusion for the interpretation of cerebral functions should be clearly understood

Suppose a CR was disrupted following a partial decortication If post-operative training reinstated the CR, it was explained formerly on the basis of the remaining functional cortex ("vicarious function") Thus, Pavlov reported the re establishment of auditory salivary conditioned reflexes in the dog, following complete bilateral extirpation of the temporal lobes On the assumption of the cortical locus of conditioning these results were interpreted to indicate the presence of acoustic radiations in the remaining functional cortex (Pavlov 1927, p 339) It is known now that the acoustic radiations from the (subcortical) thalamus project only to the auditory areas within the cortical temporal lobes Pavlov s results are recognized today as an example of conditioning involving *subcortical* acoustic centers Depending upon the nature of the process the return of function following some cortical ablation *may* conceivably involve the remaining cortex or it may depend solely upon subcortical centers (Raab and Ades 1946)

It is important to note that following cortical insult, there may be no loss in CR function whatsoever because the process was *initially* mediated subcortically The persistence of visual CR following bilateral extirpation of the striate areas in the dog is apparently due to the *initial* involvement of *subcortical* centers (Wing and Smith, 1942, and Wing 1947) This interpretation would appear to apply at least for the sensory, *visual*, component The pathway for the motor system, after sensory-cortical ablation is still to be determined If the initial conditioning were to involve a discrete "skilled" striated muscular response, the mechanism would appear to include a *subcortical sensory* and *cortical motor* pathway This problem has remained relatively unexplored (cf Whatmore and Kleitman, 1946, and Bromiley, 1948b) Direct *experimental* data of this kind with *human* subjects, so long as man remains an ethical animal, will never be obtained

The search for the locus of conditioning must, therefore, anticipate that the switching may occur either at the cortical or sub-

cortical integrating centers. One methodological approach consists in making a given neural component nonfunctional, temporarily (drugs) or permanently (surgical extirpation). Another interesting approach consists in electrically activating a given neural element. By this latter method it was found that the sense organ or sensory nerve for the CS is dispensable in the conditioning process. By a clever technique, Loucks (1938) introduced direct electric stimulation of the sensory (visual) cortex. This was adequate as the CS when reinforced with the appropriate US for salivation or leg flexion respectively. Theoretically, it is entirely possible that the response evoked by the CS at the cerebral termination (e.g., colliculus) is important. Is conditioning, even from the classical view, an "association" of *stimuli or responses*? The inaccessibility of the cerebral, reflexive motor components of the CS has precluded study of this question.

What of the unconditioned stimulus (US)? If it is applied as electrical stimulation at the final common path in the spinal cord (dorsal motor roots), a CR will not be established (Loucks and Gantt, 1938). The stimulus was sufficiently strong to evoke a response, but conditioning did not develop because the requisite neural integrator for the US and the CS was not involved. When the US was increased in intensity so as presumably to activate nearby pain fibers, a CR was developed. But this is no different from applying shock externally and directly to the sensorium. Likewise, if the CS is followed by electrical stimulation of a motor nerve activating muscular responses, the CR fails to develop (Hilgard and Allen, 1938). Similar negative results were obtained in another demonstration by Loucks (1935) in which the normal CS (such as sound of bell) was followed by faradic stimulation of the cortical motor area. Leg flexion was evoked regularly during training, but the CR did not develop, even after hundreds of training trials. The synaptic "switching," therefore, does not occur at the cortical motor outlet. One apparent exception is the reported positive result in which the electric current was applied as US directly to the lateral lobe of the cerebellum (Brogden and Gantt, 1942). Less is known in this instance of the pathways involved. It is possible that stimulation

was not strictly motor, as suggested by more recent evidence of sensory representation in the cerebellum (cf. Snider and Stowell, 1944).

A leg-flexion CR was established by Loucks in the above study when the bell-(cortical) shock combinations were followed by food. This dual role of the unconditioned stimulus was demonstrated earlier in a different experimental situation. The US serves to evoke the desired response and also offers the required motivation. Depending upon the situation, the two functions may or may not be incorporated into a single stimulus (Finch and Culler, 1934). Thus, one may lift an animal's leg repeatedly, using one's voice or gesture as the preceding CS without avail, unless a reward is applied in one form or another (e.g., petting, food). The problem of the role of motivation in conditioning brings us too far afield from the immediate discussion, but it serves to emphasize the fact that conditioning probably is much more than simple "association."

The theory of simple synaptic reflex switching might suggest that the neural pathway for the UR and CR are identical. Evidence indicating that the pathways may not be identical has been offered by Settlage (1936). He trained cats for leg flexion with shock reinforcement while the animals were partially drugged. The latter was accomplished with sodium amytal so that, during training, the UR (to shock) was produced but the CR (to the CS alone) never appeared. After the effect of the drug wore off, the CR was evoked by the CS. This evidence suggests that the UR is mediated through a different neural path from that for the CR. There is, however, one methodological weakness. Normally a CR is weaker than the UR. In this situation, the CR could have been so weak as not to be observed and would require muscle potential measurements to detect it. Such a test is necessary to prove that the muscles were completely unresponsive to the CS during training.

Other evidence which supports the conclusion that the complete pathways for the CS and US are not necessary to establish conditioning is reported by Whatmore and Kleitman (1946), who studied the role of sensory and motor cortical areas in a series of animals tested with escape and avoidance conditioning.

Some tentative conclusions are indicated. Conditioning is a complex of behavioral activities.[1] Therefore, it is unlikely that learning, even of "simple" conditioning, consists of synaptic switching of unit-reflexes.[2] The "locus" of conditioning, as a consequence, can be expected to involve some integrating cerebral ("association") area outside of the specific pathways which mediate the conditioned and unconditioned stimuli and responses.

In view of the fact that conditioning can occur in totally decorticated animals, the conditioning locus may be at the cortical or the subcortical level. The association system may very well involve both cortical-subcortical recurrent pathways, or similar reverberatory circuits at either cerebral level alone. If these latter neurological mechanisms should be found to be essential in conditioning, the search for a *locus* is futile. The neurological correlate would be a pattern or circuit, rather than some change at a given synaptic center.

Response Theory. The preceding data suggest that learning requires neural changes at some cerebral integrating, cortical or subcortical, center.[3] Another aspect of the problem is related to the *response* ("motor") *theory of behavior*. Specifically, is the unconditioned response of the effector itself essential in the learning process?

Pilocarpine, which produces salivation, is ineffective as the US (Kleitman, 1927; Finch, 1938) since it directly produces glandular,

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[1] Many of the behavioral researches since Pavlov have so modified and enriched the initial conditioning concepts as to leave little of his original reflexology (cf. Skinner, 1938, and Hull, 1943). A critique of the behavioral concepts which have evolved from Pavlov's original hypotheses is not relevant here (also cf. Hilgard and Marquis, 1940, Maier and Schneirla, 1942, Lashley and Wade, 1946, Hilgard, 1948, and Razran, 1949). Hebb's book (1949) appeared too late for serious evaluation here, and it should be consulted with respect to all the problems considered in this chapter.

[2] A possible example of "simple" synaptic switching may be that reported in spinal conditioning (cf. footnote 3).

[3] Evidence has been reported that behavioral modifications occur at the level of the spinal cord (Shurrager and Culler, 1938), but this has been questioned (cf. Deese and Kellogg, 1949). Final confirmation of spinal conditioning would not necessarily require any essential change in the present description except to extend the levels (cortical, subcortical and spinal) at which the behavioral modifications occur.

and not neural, activity. If, however, morphine is used as the US, salivary conditioning does develop (Kleitman and Crisler, 1927). Here, the morphine is effective via the requisite neural centers.

Salivary conditioning to morphine developed even though salivation was inhibited by the use of atropine during the training periods. The CR appeared after training was completed and the inhibitory effect of the atropine was gone (Crisler, 1930). Similar evidence is available in a study by Light and Gantt (1936). Training consisted of successive trials of paired presentations of sound (CS) followed by shock (US) to hind limb of a dog The limb failed to react during training because the ventral motor roots innervating it had been previously crushed. After recovery of neural innervation, some two to three months later, the CR of the limb to sound was spontaneously evoked.

Both of the above experiments contain a fundamental methodological weakness. The effect of morphine is to produce a *general nausea* of which salivation is but *one* single component Nausea responses did occur during training even though salivation *per se* was inhibited. Likewise, in the study by Light and Gantt, the effect of electric shock was to produce *generalized* responses, of which the limb reaction was but one aspect. This latter experiment was repeated by Kellogg and his collaborators and the activity of *all* limbs was objectively recorded. The evidence is conclusive that the CR under this experimental condition is a generalized pattern (Kellogg, *et al.,* 1940). The available evidence would appear to indicate that a CR cannot be developed if the (neurally innervated) response is absent during training.

An ingenious approach was made by Harlow and Stagner (1933) in applying shock-reinforced conditioning training during the *complete* paralysis of *all* striated muscles which was induced with curare. The negative results of this study are in line with the interpretation that the neurally innervated response is necessary during training. This experiment, however, had to be discounted when it was established that curare also depresses the cerebral cortex. Learning developed in the drug state is restricted to this condition and "dissociated" from the normal behavior (Girden and Culler, 1937). This

view was supported by extensive electrical measurements (Culler, *et al.,* 1939) and functional data (cf. Girden, 1947).[4]

What would appear to be an adequate test of the motor theory was made by giving *both* the training series and final tests during the paralysis. With proper dosage of the drug, training could be continued during complete paralysis. Critical tests were made after the recovery of *some* muscular function while the animal was still subjected to the drug. Acoustic and visual stimuli were used with shock-reinforced conditioning of striated ("voluntary") muscle, pupillary, and other autonomic responses. The evidence was interpreted in favor of a motor theory insofar as the establishment of conditioning with striated muscle is concerned (Girden, 1943).

The controversy over the relative importance of the central (cerebral) and peripheral (motor) components in behavior is well illustrated by the views of James (1884) and Cannon (1931). According to James, the several emotions are differentiated by the excitement response patterns. But these organic states have been shown to be identical by Cannon and Bard in manifestly different emotions such as fear and rage. For the latter investigators, the differences in the emotional patterns are determined and patterned in the cerebrum, specifically in the subcortical hypothalamic integrators. It should be noted in the last connection that electrical stimulation in animals of the appropriate thalamic nuclei will evoke separately the particular responses associated with excited emotions, but this is probably not indicative of the presence of the real ("felt") emotional experience. On the contrary, the thalamically aroused expressions of autonomic activity are stereotyped (Masserman, 1946).

The motor theory does not necessarily negate the importance of the central mechanism. But whether the pattern, to be *integrated cerebrally,* will be "fear" or "rage" will depend upon the nature of the *response* made. In the absence of response (e.g., flight or attack) in the initial situation, the final pattern would be limited to excite-

[4] A very recent study has given the pharmacological confirmation for this functional evidence of the cerebral effect of curare agents. The analysis isolated those components which produce the cerebral depression from those which effect the classical myoneural block (Sauvage, *et al.,* 1949).

ment. For the goal-directed behavior, some implicit or explicit response is necessary *during the initial* conditioning. It is *this* response which operationally defines the behavior as fear or rage. In other words, the emotional pattern in its primary instatement and organization requires both a motor component to identify the pattern and a central component to integrate it (Girden, 1943).

Whether or not the long established pattern is subject to cerebral short circuiting, such that the response is no longer necessary, is an entirely different question. It has been argued, in opposition to a response theory, that the fast runs by a pianist are evidence for an implicit cerebral mechanism. Such a coordinated pattern of activity is performed too rapidly to be dependent upon proprioceptive (kinesthetic) impulses returning from the muscles to serve as cues for the next finger action. It is, therefore, concluded that the pattern must always depend upon some central organization (cf Morgan, 1943, p. 549 f). The point is well taken but applies only to the *well established* engram. It is not pertinent to the *initial* development of the skill, during which the finger responses are exceedingly slow.

It is not at all unreasonable to expect, as suggested in the above analysis, that the response is necessary for the initial development of a "motor" skill. As far as experience indicates, the golfer, diver, pianist, must all practice, and diligently, the required motor activity. Later (with overlearning when the particular acts no longer are sharply conscious?) there may be a short-circuiting such that the pattern is controlled by cerebral pathways. Speculative as it admittedly is, evidence for such cerebrally located neurological mechanisms has been reported by Lorente de Nó. If techniques could be devised to render such apparatus nonfunctional, an important area of research might be opened up. For further detailed consideration of evidence on the response theory, consult Fearing (1930), Holt (1931), Hilgard (1948) and Lashley (1934).

Some mention should be made of the recent advances in the field of servo-mechanisms. These researches have had their immediate outcomes in human engineering and electronic computers (e.g., Eniac). In a sense, "cybernetics" (steersman or governor) is an analogue of the response theory of behavior. These "mechanico-

electrical" systems depend upon feed-back indicators, or impulses, which govern or control the activities at hand. This concept was developed from the psycho-physiological facts of the kinesthetic signals to the brain which are initiated by muscular activity (Wiener, 1948). Further implications of the work in this field will be considered in the terminal theoretical discussion.

"Complex" Processes

Maze Learning.- The approach taken to a problem will determine the nature of the resultant data; the way a question is put will color the answer which is evoked The importance of this point is readily recognized in the study of attitudes and polling studies which make use of the questionnaire technique. But this inherent implication of *operationalism* is equally important in the present connection. Thus, the nature of conditioning theory and its methodology determined the kind of question "asked" of the nervous system. One outcome was the search for the "locus" of the synaptic switching. If one makes different assumptions about the nature of psychological functions, the methods devised to test them may lead to radically different results. Some of the different formulations were developed because conditioned reflexology appeared to be inadequate at the behavior level.

An appropriate illustration is Lashley's now classic studies of maze learning in the rat. The search for the neural correlate in this case was made in a different way. Let us take a large number of rats to be trained to run a maze to a required level of proficiency. The role of the cerebral cortex in this habit is then determined by extirpating different areas in several groups of animals before or after training. The locus of the lesions was found to be inconsequential. Instead, impairment in learning correlated with the amount or mass of extirpated cortex. This relationship appeared better as the difficulty of the maze was greater (Lashley, 1929). In the same fashion, retention of the already acquired maze habit was a function of size, not locus, of the cortical lesions In short, the cortical mechanism in the maze performance by the rat appeared to act on the basis of a *principle of mass action.*

Consider one other fundamental problem, perceptual in nature, which appeared in Lashley's researches. Rats trained to a brightness discrimination will retain this function regardless of the size or locus of the lesions. In fact, the functional remnant in the striate area can be remarkably small (Lashley, 1939). In this function, the cortex is said to be *equipotential*.

Strictly speaking, mass action and equipotentiality are independent concepts. Mass action could apply whether equipotentiality were true or not. As the extreme opposite of equipotentiality one could assume specificity of function in the cortex and still accept the "law" of mass action. Suppose that different auditory pitches are sharply localized in the several parts of the rat's auditory cortex (an assumption highly untenable and probably completely false). One could now demonstrate mass action in the disturbance of the auditory localization habit for noise-stimuli as more and more auditory cortex were, bilaterally, removed. Since auditory localization for pure tones is well nigh impossible in the rat, the cortical intervention would, in effect, reduce the complexity of the stimulus.[5] The same results would occur if the cortical acoustic area in the rat were completely equipotential; in other terms, if the acoustic system operated as a telephone system The experimental artifact *in both cases* would be in the nature of the stimulus used. The true test of equipotentiality in this particular illustration would require a pure tone stimulus (cf. Pennington, 1941)

The applicability of the principle of equipotentiality, therefore, would depend upon the nature of the behavior. Suppose running the maze involves a complex of *specific* "abilities." If these functions were "located" in different cortical locations, there would be greater impairment of the habit as more cortex were extirpated It would mean that all the specific functions contributed to the maze habit. Such a possibility is suggested by the results of Ghiselli and Brown (1938) who have reported that interference of the maze habit increases in the rat with greater lesions in the sensory thalamus. In this specific case, it is not that the cortex is equipotential, but

[5] The same result would obtain if the cortex remained intact but the complexity of the sounds was reduced with the appropriate filters.

that progressive impairment of sensory functions interferes with maze learning. It would be equally misleading to "explain" this situation on the basis of mass action.

Part of the difficulty arises from a still inadequate understanding of the nature of psychological processes. Is the maze habit a concatenation of specific abilities or are there also "general" factors involved? This type of problem is strictly psychological in nature and beyond our scope. Mention should be made, however, of the finding that, other things equal, cortical lesions affect learning in duller rats more than in brighter rats (Erickson, 1939). As a final caution, it is now recognized that mass action and equipotentiality are inapplicable when speaking of precise sensory-motor functions, especially in the higher phyla (e.g., monkey, man). In the latter, localized lesions in the appropriate cortical area will affect finger manipulation.

With all these limitations, the concept of equipotentiality is nevertheless a most appropriate description of certain fundamental processes. As already noted, brightness discrimination in the rat does not depend upon particular cortical neurones. So long as the required minimum amount is intact, the optic cortex is equipotential in maintaining this function undisturbed. This situation holds for the gamut of processes which are *relational* in nature, such as equivalent figures (Klüver, 1933) and pattern vision (Lashley, 1939).

The fact that there is some evidence of neurophysiologic equipotentiality for perceptual processes in the rat should not lead to an immediate extrapolation of this concept to all human integrative mechanisms. The inference, for those perceptual processes which are relational in nature, is admittedly strong, although the direct neurophysiologic evidence in man is far from complete. There are other processes, however, of considerable importance in primates which are only primitively represented in the rodent. This is particularly true of so-called symbolic processes.

Symbolic Processes. – The importance of ("mental") set for symbolic processes may be inferred from the striking contrast in the capacity for delayed reaction between the rat (Hunter, 1913)

and monkey (Tinklepaugh, 1928). Whatever other apparatus is important for this performance, the presence of the frontal lobe appears required. The first experimental investigation of the role of the frontal lobe in the cat suggested its importance in the acquisition of new activities such as those demanded by the problem box, which was used by Franz in his researches (1907).

The frontal cortex appears to be essential for the delayed reaction in the monkey. With complete bilateral extirpation, the capacity is extremely limited in this animal. Like the normal rat, the operated monkey must be set toward the correct goal; even then, the delay-interval can be only very brief or errors occur. Complete bilateral extirpation of these areas also abolishes the delayed alternation solution (Jacobsen and Nissen, 1937).

The results from rat to monkey suggest that the "trace" for the delayed reaction set is mediated through the frontal areas. But Malmo's repetition of Jacobsen's study makes it necessary to modify somewhat this interpretation (Malmo, 1942). By extinguishing the light during the delay period, the operated monkeys could still react correctly. The surgical intervention apparently interferes with "set" as the animals were easily distracted This has led to the distinction between *expectancy* (motor, expressive function) and *trace* (sensory, receptive, "imaginal" function). The posterior part of the cortex (with recurrent circuits?) is presumed to serve in trace functions and the anterior, frontal, lobe mediates expectancy processes.[6]

The effect of frontal lobectomies, or lobotomies (Freeman and Watts, 1942) in humans appears to be in line with this hypothesis. The preoperative anxieties are avoided by eliminating expectancy, since there appears to be a neurological organization involving the frontal lobes and the subcortical apparatus mediating emotional behavior.[7] An organization of related human functions into recep-

[6] In terms of this distinction, the disappearance of the alpha rhythm upon the onset of an attentive set to visual stimuli or intellectual tasks exemplifies the trace component of this process. The report that the alpha may persist during attention to auditory stimuli (Adrian, 1947) suggests that the attentive trace process is not simple or unitary.

[7] Whether the overall benefit to the patient balances the resultant loss in function following such operations is a clinical question not pertinent here.

tive and expressive components is given by Weisenberg and McBride (1935). A comprehensive discussion of expectancy is to be found in Hilgard and Marquis (1940) and in Morgan (1943).

The limited capacity of the rat for delayed reaction would properly lead one to suspect that symbolic processes are equally primitive in the rodent. It is not surprising that the double alternation problem (LLRR) is an extremely difficult if not impossible task for the rat. Frontal lobe development in the rat is decidedly poor, as is cortical development in general. The "reasoning in rats" (Maier, 1932, a, b) is most primitive. Here, the advantages of animal neurosurgery in conjunction with controlled experiments must be relinquished for the less certain human clinical data. But the nonexperimental clinical evidence must be accepted with reservations. It would be rare, indeed, to find *pre-operative normal* tests obtained on a patient requiring a brain operation. One cannot anticipate a cerebral gunshot, tumor, or infection. The normal, pre-pathological, function must therefore be inferred, although on occasion such judgments have a high level of confidence (Hebb, 1942).

Tumors and other pathological conditions often have effects far removed from the site of the tumor or the focus of the infection. The pressure produced by the tumor can injure far distant cortex and yet manifest no observable macroscopic effects. Infections can produce similar distant injuries systemically. Again, unlike experimental animal researches, human post-mortem examinations are not a regular practice. When the operation is successful, the extent of the pathology can only be approximated from pre-operative X-ray plates and drawings or photographs made during the operation.

As could be anticipated, several different views have been offered to explain how the cerebral cortex operates in man. Some would argue for strict projection. Goldstein, on the other hand, inclines toward equipotentiality, except for sensory-motor functions (1939). Tentative as judgments must be, for the time being, there is something to be said for the position taken by Head (1926). Memory function may depend more on one area than another, but almost

any (complex) function involves many aspects of behavior which, in turn, would require total cortical activity.

The term "vicarious function" in its broadest implication makes no contribution. There is no good evidence that a new region "takes over" for what had been maintained by the now extirpated areas. Other systems must be present which are also possible of accomplishing, as well or in some modified way, the same function or behavior. If a rat has been trained to run a maze, it will "know" the maze even if all its limbs are subsequently amputated. It would "run" the maze accurately by rolling down the alleys. Nothing has taken over the "running" ability of maze habit because the "habit" was perceptually organized. Rats relearn brightness discriminations following bilateral extirpation of the striate areas not because some other cortical areas take over visual functions, but because of the presence of the subcortical optic system.

From the neurophysiological point of view, the "nonspecific" cortex may operate by strict localization of function, on the basis of equipotentiality, or a combination of both. It was suggested earlier that a major difficulty has been the lack of fundamental insight into the psychological *nature* of symbolic processes. What is intelligence? Is it composed of many specific abilities, a general factor or both? The resolution of this strictly psychological problem will give the precise test-instruments needed in studying the neural correlates of such processes. If "brightness and dullness" in the maze habit is solely a general factor, then equipotentiality will have some meaning. If, instead, the rat's behavior here is composed of a number of specific abilities, then precise tests of these functions are required in order to determine the role of the cortical apparatus in this behavior.

It is precisely this sort of difficulty that arises in evaluating the distinction by Goldstein between "concrete" and "abstract" processes, suggested as a consequence of his study of brain injured patients (1940). It is not at all unlikely that a real contribution may be made in this general direction by means of factor analysis (Thurstone, 1948). Until then, the interpretation of cerebral functions must remain relatively uncertain in this respect.

Some Theoretical Perspectives

In the present uncertain state of understanding, both of psychologic and neurologic integrative mechanisms, an examination of some theoretical viewpoints may not be amiss. Throughout, one should be careful to distinguish between argument by fact, inference, or analogy. For purposes of exposition, the views are classified as "molecular" and "molar," and deal primarily with the problems of relational properties and retention.

Reflexive Molecular Theory. It has been said somewhere that were all of Freud's specific hypotheses eventually proved invalid, his place and importance in the history of psychology are nevertheless firmly established for all time. The same may be said of Pavlov. He was not a psychologist and not interested in psychology. He devised the salivary conditioned reflex (CR) methodology for the purpose of determining the physiological principles of the cerebral cortex. The salivary CR is used in but few laboratories on this continent, whereas his physiological concepts of cortical mechanisms have been converted into behavioral concepts and hotly debated in psychological theory.

Pavlov's hypotheses were directed specifically to the cortical apparatus and the concepts of cortical excitation and inhibition form the core of his theory. The sensoria are considered to project, point for point, to the cerebral cortex. (The problem of tri-dimensional vision was ignored.) Neural activity aroused by unconditioned stimulation (US) would terminate at a given cortical sensory "analyzer," from which there would be a spread of excitation, diminishing in intensity from its primary focal point. The successive activation of two focal points repeatedly, by the conditioned (CS) and the more intense unconditioned (US) stimuli, would result in some sort of drainage of the neural excitation produced by the CS to the greater focus of neural excitation produced by the US. The end point of this process would be the instigation of the unconditioned response (UR) by the conditioned stimulus (CS) alone, i.e., a conditioned reflex (CR). During the conditioning, there is a progressive increase in concentration of the cortical excitation. This

occurs because of the progressive inhibition of the initial irradiation. Successive presentations of the CS alone would produce extinction, the excitation process being replaced by one of inhibition. This description is not a factual account of the actual cortical physiology but, rather, inferred by Pavlov from the behavioral data found in his experiments. It has been suggested that, in one sense, it is a cortical counterpart of a drainage theory.

According to Pavlov's theory, there is thus developed in animals subjected to a number of conditioning situations, many (a "mosaic" of) focal concentrations of excitation and inhibition, representing the variety of patterns which were developed in the training (or experiential) situations. For Pavlov, sleep would occur because of inhibition of the activity of all of the cerebral cortex, and hypnosis because of a less extensive inhibition.

Irradiation would constitute the basis for psychological "generalization" and much effort has been directed to an assay of this hypothesis. Loucks has evaluated Pavlov's own data on this question and concluded that not only is it not evidence for irradiation, but inconsistent with Pavlov's own interpretation (cf. Loucks, 1933, 1937).

The assumption of spatial cortical irradiation as the basis for sensory generalization is inconsistent with the fact that the peripheral neural base for sensory intensities involves frequency of discharge of nerve impulses, and not merely spatial position. Actual physiological tests of cortical activity by Dusser de Barenne and McCulloch (1938) also contradict the Pavlovian hypothesis. These investigators used cortical strychninization with electrical recording and have reported that the irradiation was neither uniform nor consistent, which is not in accord with Pavlov's supposition.

The validity of Pavlov's theories does not necessarily depend upon the assumption of a cortical locus for learning. His hypotheses could apply with equal force to subcortical centers. The theories prove inadequate because of even more important physiological and behavioral evidence already discussed. Other molecular approaches, reflexive in concept, lay their emphasis on peripheral rather than central neural components. In so doing, there is no denial of the

importance of the central contribution Rather, in one form or another, the insistence is upon the equal importance of the peripheral apparatus. One aspect of this problem has been discussed in connection with the motor theory of conditioning Extended treatment of this question is beyond our scope, since many of the data are strictly psychological in nature

Field Molar Theory.—The gestalt, field, theories in psychology originate with Wertheimer and his colleagues, whose important studies of the organized properties of perceptual processes have done much to enrich psychological research and theory As active opponents of reflexology, they have demonstrated relational properties which would appear to be inexplicable by molecular concepts In line with this school, Lashley has demonstrated that *relational properties* are independent of the particular structure which is activated (Lashley 1941, 1942) A square is so recognized regardless of variations in size or area of retinal stimulation Many perceptual and possibly intellectual, functions are relational in nature (cf Kluver, 1933) Whatever the outcome of the theoretical controversy between the molar and molecular approaches, it is extremely doubtful the neural engrams for such experiences will, in any case, be found to have specific cerebral loci The neural mechanism will be a pattern, whether as a volley of impulses, recurrent circuits, or an isomorphic field

The basis for gestalt cerebral theory is simply stated It starts with the assumption made by all sciences that the physical world is subject to natural law It follows with the application of the same principles to man as part of that world No one would take serious issue thus far The additional assumption is made that *field* theory is the most appropriate description of the physical world, and the inference is made that cerebral dynamics operate according to these field principles The real test of gestalt theory, therefore, must be a physiologic determination of the principle of isomorphism (Köhler, 1940).

Isomorphism is defined as, "The structural correspondence of excitatory fields in the brain with the experienced contents of con-

sciousness" (Warren, 1934, p. 145). Fruitful as gestalt theory has been in psychology, especially in the field of perception, little evidence has been procured until recently with respect to this keystone, isomorphism. This has been due largely to the lack of suitable electrical apparatus.

The known facts of nerve physiology, the neurone-unit, the all-or-none law, do not necessarily preclude the possibility of explaining cortical phenomena by field theory. The neural integrative mechanisms within the gray matter of the brain may not be identical with that of one-way transmission in the peripheral nerve fiber. The all-or-none law and one-way transmission are phenomena of "neurone-synaptic" organization, whether in the central or in the peripheral nervous system. The unmyelinated cerebral gray matter, however, may function like the *nerve-net* which behaves unlike the insulated neurones. When such unmyelinated neurones are adjacent to each other in a conducting medium, impulses in one will alter the excitability in the other (Arvanitaki, 1942). And Libet and Gerard (1941) have reported that the intercellular currents are important in synchronizing action of many neurones. The anatomic evidence of Lorente de Nó (1934) also indicates that cortical relationships may involve a diffuse spread of excitation unlike the all-or-none nature of the nerve impulse. Whether such excitations manifest field properties is still to be determined. The researches of Burr have been applied in support of a field theory (cf. Northrop, 1947, 491ff) and, very recently, Kohler and Held (1949) have reported results with human subjects. Definitive data may be forthcoming in the very next few years.

It is important to note that isomorphism is based on a particular concept borrowed from physics. This is in a sense double-jeopardy. Serious argument could be raised, and by physicists, whether the theory of fields is the best description of physical phenomena. In fact, to the contrary, there is much in physics which is not in accordance with field theory. An important area in modern physics deals with what in psychology has been criticized as the molecular approach, i.e., *quantum physics*. One controversy in the history of physics centered about the particle versus wave nature of light. New-

ton believed light to be composed of particles. Later, it was "proved" that light was a wave phenomenon because it could be diffracted. Since the development of modern quantum mechanics, the distinction between wave and particle is beginning to disappear. Light and other physical phenomena manifest attributes of both waves and quanta. Satisfactory mathematical descriptions have been found which resolve this discrepancy between field theory and quantum mechanics, although their application is in general difficult. In any case, it is clear that field and quanta are complementary concepts.

The assumption that cerebral dynamics operate by the same laws as those of the physical world is serious enough. But to make the more questionable jump to field theory as *the* most appropriate description of psychological processes is certainly to be questioned. This is especially pertinent in view of the present status of fields in physics as being only a partial description of nature. Hence one may seriously question the desirability of such a double assumption.

There is no physical-psychophysiological à priori reason, therefore, why *field theory* must underlie the assumption of isomorphism One could, perhaps, with equal justification, as readily assume the isomorphic hypothesis on a quantum basis Illustrative of this sort of argument by analogy is the striking similarity in the concept of quanta and the all-or-none law of the nerve impulse.[8]

The fact of psychological patterns or "fields" does not preclude a "molecular" substrate. Some psychological functions are susceptible to explanation in terms of known (molecular) principles of neurophysiology. Intensity of experience is a function of the number of impulses as well as possibly the increase in number of fibers activated. For that matter, "patterning" of the rate of discharge of nerve impulses appears to play a role in the *quality* of experience (Erlanger and Gasser, 1937; and Harthne, 1937, 1938). *Patterns* or volleys of discrete nerve impulses are spatially or temporally represented in neurone chains. Sound "waves" are successive rarefactions and

[8] This sort of speculation is perhaps worthless, but it does suggest, *by analogy*, that the molar-molecular controversy in psychology may be somewhat exaggerated for no useful purpose. It may very well be that our theoretical orientation in this controversial aspect is handicapping our present progress.

condensations of particles of air. When they possess a particular organization (e.g., the required frequencies) they will arouse, in given groups of neurones in the auditory nerve, volleys (patterns?) of impulses which will underly the experienced chord or tonal pattern.

It is possible, too, that the psychological processes which are relational in character also have a neural molecular base. It has been suggested that "the problem of gestalt and universals" is capable of physical solution (Wiener, 1948, p 166) The requirement of a ("television") camera to scan a page of writing and "translate" it into, say, appropriate sound patterns (so that the blind could "read" by hearing), is the essential problem of perceptual relations. That this kind of "molecular" solution is not at all impossible is suggested by an analysis of the theoretical considerations by Wiener (also cf. Adrian, 1947). Actual research directed toward the solution of this problem is now in progress (Fitts, 1949, p 50) While this is, again, an argument by analogy, it is of interest that the concept and theory arose from the available evidence of cerebral ("molecular") neurophysiology.

Regardless of the theoretical framework in which the learning process is considered, there inevitably arises the problem of the nature of the neural correlate of retention. This issue must be faced in dealing with any processes involving learning. The earlier writers hypothesized a change, reduction, in "resistance" at the synapse as behavioral modifications occurred The behavioral theories, in one form or another, imply a synaptic change The "stamping in" through the "law of effect" and the analogy of Pavlov's conditioned reflex to McDougall's drainage theory are illustrations of this sort. The molar theorists have not paid too much attention to this problem, although the same question arises with respect to the relational perceptual process, i.e., what makes for its revival, as in recognizing or remembering a given pattern at some subsequent time?

The treatment, in general, has remained largely at a speculative level, primarily because no technique is available to make *functional* tests at the synapse before and after (let alone *during*) a given learning experiment. It is conceivable that, by the use of suitable radio-

active tracers and the electron microscope, a useful weapon might be developed for this purpose One of the earlier theories, which shows signs of renaissance, is based upon the assumption of actual growth of neuronic endings during learning (Kappers, 1917 and Holt, 1931). Theoretically, there is much to be said for some view of this sort. Ultimately, any valid theory will require functional proof, however it may be obtained.

Whatever the eventual theoretical outcome, cerebral activity is as "natural" as any other process of the living organism The "function" of the nervous system is to conduct impulses It is "irritated" by sense organs, the successive neurones through the cerebrum are stimulated, and there is a final activation of muscles or glands This process is then reinstated repeatedly, probably accompanied by intracerebral (recurrent) patterns It is a *good assumption* that "the brain by itself and apart from the rest of the body initiates nothing None of its activities is 'spontaneous' in the sense of being self-starting" (Herrick 1929, p 119) Its "purpose" is to conduct impulses, complex as that process may be This form of "mechanism" is to be distinguished from the connotation given by some psychologists, for whom this term is identified with the "mechanical reflex" Rather, the emphasis is upon mechanistic as natural, deriving its behavior from natural laws The problem of conscious behavior would also fall within such a framework as an important phenomenon to be investigated (cf Herrick, 1926)

The assumption of the natural nature of psychological events needs explicit reaffirmation from time to time, lest there be a serious distortion in perspective One can with a microscope, get so close to a leaf as to overlook the forest

The "search for consciousness" may be another example of the fruitless quest for the fountain of youth Because the experience is evoked with appropriate cortical stimulation (sensory area) gives no assurance that the functional locus is in this cerebral terminus One has simply contacted one point in a complex circuit All of the components, beginning with the sense organ, contribute to the activity. In fact, it is equally possible that the "locus" is in recurrent or the, still inaccessible, subcortical reflex pathways If consciousness psy-

chologically is relational in nature (Boring, 1933), the neutral counterpart could hardly be a "point-locus." In the same vein, the fundamental error of phrenology was Gall's assumption of a faculty psychology. The questions in such cases are wrong, because they were improperly put. The search was for a something which does not exist as a *thing*, but functions as a series of complexly constructed *events*.

Sherrington, in evaluating his life work in neurophysiology, concludes that brain events and mind-experiences are incompatible. "The two for all I can do remain refractorily apart" (Sherrington, 1941, p. 312). There would appear to be no better way to conclude here than with Lashley's answer to this judgment

> "With this statement the greatest living neuro physiologist despairs of finding a common ground between the sciences of the brain and of mind. I am confident, however, that the outlook is not so hopeless as Sir Charles believes. In fact he seems to have missed a solution of the problem by no more than the turning of a page. In the same lectures he has faced the problem of the nature of life and has found that life is not a thing attached to this or that substance or chemical action, but is organized activity, varying in character with complexity of structure and ranging without discoverable discontinuity from the nearly crystalline simplicity of the filterable virus to the elaborate organization of the mammalian body He has just missed seeing that mind also is not a thing attached to life, a unique form of existence, but is a term including an indefinite number of complex structures or relations" (Lashley, 1941, pp 461f)

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CHAPTER 4

DEVELOPMENT AND MATURATION

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Much of psychological theory has been concerned with adult behavior, or with the functions of the minds of adults. However we are becoming increasingly aware that an understanding of psychological functions is greatly enhanced by knowledge about the processes through which these functions develop and change with the age of the individual. Research during the last thirty years has given us a large body of facts about the development of behavior, from the first fetal movements throughout the life span. It will be impossible, here, to review all of these facts, or even to discuss the developmental aspects in all the fields of psychology. But an effort will be made to characterize the general nature of development and maturation on the basis of current knowledge in several broad areas.

The facts may be best described within the framework of several generalizations which are not independent of each other, but nevertheless emphasize different aspects of the developmental process. (1) The growth of an organism is a function of the strong developmental impetus of a specific genetic inheritance with its innate abilities and potentialities, in continuous interaction with a complex and varying environment. (2) This interacting process starts with the organism, both structurally and functionally, at a relatively simple level, and becomes increasingly complex during the period of infancy and childhood. It follows, furthermore, that with this increasing complexity, the organism differentiates into increasingly specialized structures and functions which become, during the early part of this process, progressively more independent of each other. But there are limiting factors which operate to *mature* a structure or function, so that at a given stage of maturation it will cease to grow or differ

entiate further. (3) These processes of differentiation and maturation within the individual are not all of a piece. Different functions mature at different stages of the organism's development. Their cycles of growth are timed differently; some functions develop concurrently, some follow each other successively. Some develop rapidly, others more slowly. As a result, different structures and functions, with their different time schedules reach their maturity in the same person at different ages. (4) There are also, concurrently with the processes of differentiation, integrative processes which organize the various factors into smoothly functioning reaction patterns, or behaviors. (5) There are individual differences in the rates at which persons go through these various processes of growth, as well as individual differences in mature capacities.

(6) These differences, both in rates and in capacities (or mature structures and functions), are brought about by many differing combinations of both inherent and environmental factors. Some factors are more susceptible than others to environmental influences. Some conditions of the environment are more potent instruments than others for effecting changes in the human organism And some persons are more vulnerable than others to the impacts of their environment. Furthermore, the effectiveness of a given environmental factor in altering an individual is often dependent on his age, or the stage of his development in the factor concerned.

It will be possible to find supporting evidence from research for some of these general principles; for others, there is as yet little material which is relevant for either verifying or refuting them. They have grown, in part, out of the writer's effort to resolve some of the apparent contradictions which have arisen in the nature nurture controversy. Is the IQ constant? If not, does it vary in any consistent way? What causes the IQ to change? Is mental development dependent largely on inherent capacities, specific opportunities to learn, or the emotional climate? Are there certain ages when environmental influences are more important than others, in directing the course of development? How can we account for individual differences? Are emotional characteristics of personality innate?

Why does one person "break" under emotional stress, and another not, under similarly trying conditions?

There is usually a tendency to plead special causes and to ignore the evidence from other sides of the controversy. Educators and therapists are more often inclined to assume that environment is all-important and it is their business to remold the inadequate and the maladjusted, by diverse educative and therapeutic methods. If the pupil or patient does not respond, then better educative and therapeutic devices are sought. Persons who take this attitude often defeat their own avowed objectives by failing to admit, or to understand, the inherent characteristics of their subjects. A thorough knowledge of one's materials will make possible the devising of more efficient and effective ways in which to encourage healthy growth and adjustment.

The wide variety of capacities and reaction tendencies in mature adults may be seen, according to the general principles suggested above, as the resultants of developmental processes in which organisms with specific genetic potentialities are growing, differentiating, and integrating, in interaction with very complex environmental influences. This developmental process may be divided temporally, on the basis of conditions peculiar to each stage, into growth of the embryo, the fetus, the child, the adolescent, the adult, and the aged. The lines of demarcation between these "stages" are arbitrary, however, because the changes are characteristically continuous, and often gradual. It is thus difficult to pick out an exact age, or degree of maturity, which separates one stage from another. Even the event of birth, which is the most exact dividing line in the series, may occur at any point within a fairly wide range of degrees of maturity of the fetal organism.

Definitions. The terms "growth," "development," and "maturation" have been used, to a considerable extent, interchangeably (Meredith, 1945). However, it seems most useful to give somewhat specific and different meanings, or at least emphases, to each, as they are used to discuss both the physical and mental changes which occur during infancy and childhood. Thus, in the present discussion, *growth* will be used primarily to describe increments in size or

amount, although sometimes it will be used as a more generally inclusive term; *development* will refer to elaboration or increasing complexity of structure and function; and *maturation* will indicate progress toward a relatively stable, adult, or mature (completed), structure or function (American College Dictionary, 1949).

As here defined, none of these terms is used to indicate any distinction among the forces operating to direct the processes. Both genetic and environmental factors determine the growth, development, and eventual maturation of an organism, and of structures and functions within the organism. If we define these terms in this way, it should prove easier to approach without bias the study of each developing function, and to attempt to understand all of its aspects, including the roles played by hereditary and environmental factors.

Some Basic Considerations of Physical Growth.—The functional, or behavioral, aspects of an organism are always limited and directed by its structure, i.e., its physical and biochemical organization. It is to be expected, then, that in growing children the physical aspects of their growth will often make a difference in their behavior. Students of developmental psychology are in particular aware of the predominating importance of physical maturity in determining the limits and possibilities of behavior in the embryo and fetus. Furthermore, a child's attitude toward his own body (including his size and degree of maturity), and also the attitudes of others toward it, are important factors in his personality, and may affect the adequacy of his social adjustments. However, there is another reason for studying the processes of physical growth Knowledge about the nature of these processes may very well afford insight into the manner in which growth, development and maturation can be expected to take place generally, mentally as well as physically. Thus we may explore profitably the ways in which mental development is similar to or different from the more objective and hence more readily measured physical aspects of growth and development.

The general pattern of growth in size and body proportions, over the entire life span, appears to be in many ways analogous to the pattern of mental development, although there are some important

differences. Growth in size, as represented by total length, is shown in Figure 4.1. The most rapid changes have already occurred prenatally, but immediately after birth the growth is still far more rapid than at any time later in life. There is a pre-adolescent spurt of rapid growth in height which lasts for about three years. Then, with the approach to maturity, the impetus to grow diminishes rapidly and

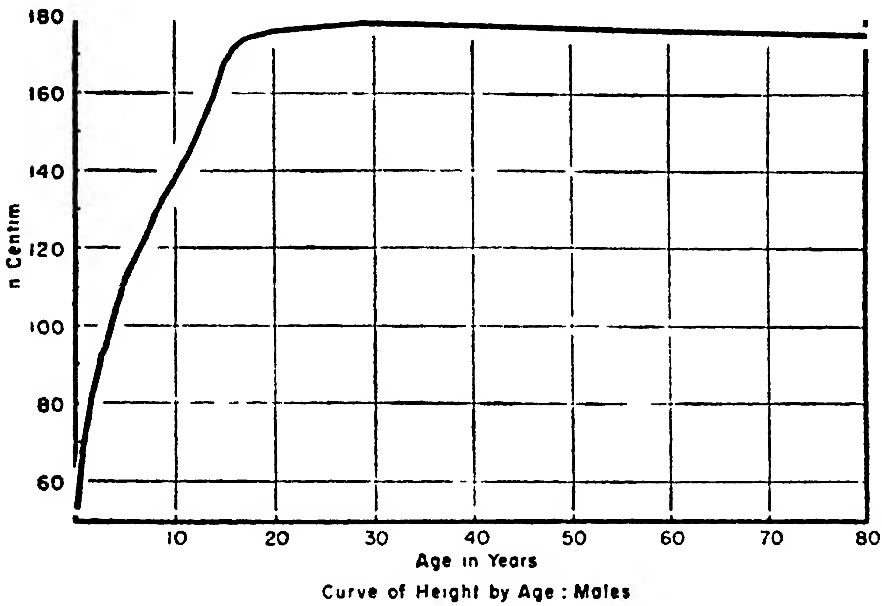


FIG. 4.1 Curves of height by age from birth to 80 years. The curve is based on three sets of data. For ages 4 through 17 years the heights are averages reported by O'Brien, Girschick and Hunt (1941) for 16 states and the District of Columbia. These have been extended downward to one month, and upward to 20 years by the use of percent of mature height for age as determined from the Berkeley Growth Study (Bayley, 1947). The curve after 20 years is estimated from the trend of Weymouth's (1925) curve.

there follows a long period of stability, with some slight actual diminution of size in old age. Similar age trends are found for growth in most physical dimensions. However, different parts of the body have different periods of rapid and slow growth, so that the proportions of the body are changing continuously throughout childhood. Also some parts, such as the head, do not have any spurt of rapid growth in adolescence. Some structures, such as the skeleton, are

much more stable in their growth trends than others, such as fat. The skeleton, although it may cease to grow under given conditions, does not diminish in volume attained. Fat, on the other hand, increases and diminishes with changing physiological, nutritional, maturational, and other environmental conditions.

We may expect to find, in the different aspects of mental growth, parallels to the above described patterns of physical growth. Thus the curve for height in Figure 4.1 may be said to approximate the general pattern not only of physical, but also of mental growth and development, as described in our first general principle. But, as in the second principle, this growth in size occurs only with differentiation and elaboration of physical structures and functions, starting with the fertilized ovum and culminating in the adult human. Although basically highly interdependent, the various systems and structures of the organism function in specialized ways which are in many respects unrelated, both in the nature of their functions and in their efficiency. These characteristics of physical growth have long been accepted, and are common knowledge. Similar characteristics may be expected in mental relationships, both among the mental variables and in mental-physical relations.

Our third principle, that different structures and functions of the organism mature at different rates and times, is very well exemplified in physical growth. There is an orderly pattern in which growth occurs. For the purpose of analogy, some of the more obvious aspects may be suggested here. As a broad generalization, it has been found that the developmental process is most precocious at the head, and spreads from there in a cephalocaudal, and also in a proximo-distal, direction. The head has most of its growth during the fetal stage so that at birth it is relatively very large, while the appendages are relatively very small. During the first year, the body gains rapidly; in the second year, the growth of the legs and arms begins to outstrip the body (Bayley & Davis, 1935). As adolescence approaches, there is an increased growth impetus [1] which accompanies the development of both primary and secondary sex characteristics. There is differ-

[1] The function of the growth hormone appears to be supplemented at this time by the rapid increase of sex hormones, the androgens and estrogens (Bayer & Bayley, 1949).

ential growth in the size and proportion of the skeleton, of the musculature, and of the amount and distribution of fat. As these changes continue, boys and girls take on their characteristic adult masculine and feminine builds.

Fourth, the physical growth processes reach a period of stability in the late teens and remain with little discernible change for a number of years. The physical aging process is also differential, some structures and functions deteriorating before others. Over the years, and gradually, muscles and tendons become less elastic, bones more brittle, sense organs less acute; hair loses its pigmentation, teeth decay, and so on. With extreme old age or senility, the physical deterioration again sets obvious limits on mental functions and on behavior, generally, as in early infancy.

Fifth, children mature physically at different ages. Within the normal limits of growth some girls have their adult height by the time they are thirteen years old, while some boys do not stop growing until they are twenty-two or twenty-three. The age of physiological maturing averages about two years earlier among girls than among boys, but within each sex there is a normal range of four or five years.

Sixth, the more obvious environmental variables, such as nutrition and disease, are known to have differential and selective effects on the various body tissues. Also, people of different ages are susceptible to different diseases, and the debilitating effects of inadequate diet are more severe at some ages than at others. These factors may influence not only size, but also *rates* of growth and maturation.

General Characteristics of Early Behavior Development.— The first behavior is so much a function of the organism's stage of physical development that the two cannot adequately be described independently. The pattern of psychomotor differentiation may be seen to start with early fetal activity. This differentiation is of two kinds, the vegetative, starting with the heart beat, and simple body actions which involve the skeletal musculature, starting with a neck bend.

According to Carmichael (1946), the fetal heart starts beating in the third week of gestation. However, it is a preneural, inde-

pendent action of the heart muscle, and not a form of neuromuscular behavior. The youngest age at which Hooker (1943) was able to get a response to stimulation was at eight weeks: the fetus bent its neck laterally, moving the head away from a hair which was pushed against the area of the cheek. With increasing age these movements gradually involved more of the body, with trunk bending and arm movements, and occurred in response to stimulation of areas progressively farther from the face. By fourteen weeks, this stereotyped response pattern had disappeared and many discrete reflexes could be elicited. New reflexes kept appearing until by twenty-five weeks most of the reflexes necessary for postnatal life were present.

We must infer from this behavior that there are already, in the eight-week fetus, some simple proprioceptive sense organs or sensory neurons which are sufficiently mature to respond to pressure stimuli, and that there are also afferent and efferent nerves, and muscles, which are capable of functioning together in this simple reflex act.

Birth is an event which occurs normally when the fetus is forty weeks old. But the general structural-functional development is such that the infant may be viable as young as twenty-six weeks (180 days). That is, the respiratory, circulatory, and digestive systems are mature enough that the infant can get its oxygen and nourishment as an independent organism rather than by way of the maternal placenta. On the other hand, the fetus can also continue to live in utero for some time after forty weeks. There are records of post-mature infants born alive with a gestation period of fifty weeks. As Carmichael (1946) has pointed out, the gestation age at which an infant may be born and live can vary, if we include the extreme cases, as much as 154 days, or approximately five months.

In the neonate (newborn infant), the neurological structure is more completely developed than the skeleton and the skeletal muscles. This is in accord with the fact that the part of the body which encases the central nervous system is relatively more nearly its mature size than are the other parts.

But neural development, although precocious with respect to the rest of the body, is by no means complete at birth. On the structural side, certain neural pathways are not yet myelinated. On the

behavioral side, there is evidence that neural impulses tend to spread, so that almost any stimulus will result in rather diffuse activity of the entire organism. This has been described as "random" or "mass" activity. For example, the new-born infant, if touched lightly, may tense and stiffen his entire body. Or a tap on the left knee with the patellar hammer may bring about the patellar (knee-jerk) reflex of the right knee, as well as generally increased bodily activity.

For some time before birth the neural and muscular development makes sensory experiences possible. The infant's own internal environment affords him experiences with proprioceptive sensations as he moves. Furthermore, these movements themselves may relieve cramped positions, thus giving rise to additional proprioceptive sensations. Also, through his movements as well as his mother's movements and postural changes, the infant may receive sensory stimulation from contact pressures on the skin. Such sensory stimuli are, of course, continued in postnatal life, when they are added to by new proprioceptive stimuli which result from the new environment. Among these new environmental factors are freedom of movement, breathing, and digestive processes. New contact sensations due to stimulation of the skin are afforded by the air, clothing, handling, bathing, and so on. The event of birth also permits stimulation of other sense organs which have matured enough to respond to some extent, but for which there had been no possible, or else very slight, stimulation in the uterus. Vision, hearing, taste, smell, feelings of warmth and of cold all become possible, environmentally. The infant consequently starts to build up experience in these new areas, responding to all of these senses, and thus forming perceptions at the same time as sensory acuity increases. As the senses become both more experienced and more acute they will play an increasing role in the child's mental life—both intellectual and affective.

The senses, however, do not function discretely, but probably contribute to a rather general and vague *gestalt*, on the experiential side, and to more or less organized patterns of reflexes on the behavioral side.

For example, one of the best organized behavior patterns is the vital process of nursing. From the intensity of the normal infant's

reactions, we may assume that it soon becomes a potent and engrossing experience. Hunger contractions of the empty stomach, and the accompanying heightened bodily activity which eventually includes crying and general signs of distress, must give rise to strong and pervasive proprioceptive sensations. Nursing quiets the infant: he calms down, appears to be "satisfied" and comfortable, and falls asleep. This rhythm of hunger and satiation becomes the dominant experience of the neonate's waking life. If his food does not agree with him, he may have additional distress from gas pressures--again proprioceptive or pain sensations- which are a part of this important feeding rhythm.

Thus, the neonate's sensory-motor and perceptual development stem from, and his emotional preoccupations and drives are predominantly directed toward, these experiences of hunger, nursing, and satiation, with their related activities The Freudian emphasis on the infantile oral-anal stage of the emotions is based on these conditions of infantile development i e , the prominent place held in the infant's life by the processes of ingestion, digestion, and elimination which are continuously repetitive, and on which survival depends Other sensory experiences in the very young infant are more likely to be few and brief, in part because maternal care keeps him in a relatively constant, unstimulating environment, and in part because he sleeps most of the time when he is neither hungry nor nursing.

Of course we have no way of knowing to what extent these early "sensory" experiences are *conscious*. It may be assumed that consciousness appears only dimly at first, and develops gradually along with the developing physical structures and functions. Probably the sensory reception of stimuli and the neuromuscular reactions to them are basic to, or at least foster and advance, the first dim conscious awareness.

Part of the process of early development includes staying awake for an increasing proportion of the time. These longer waking periods afford ever more extensive experiences of the environment, for all of the senses. This also means reactions to the environment, which reactions in turn give new experiences.

The waking experiences at first are limited also by the infant's limited ability to respond, including his lack of control over his own position. He lies supine, or as he is placed, and is unable to alter his position except by squirming his body, turning his head, and extending and contracting arms and legs—all at this stage in relatively unorganized patterns. These activities are at first reflexive in nature. There is good evidence that they come under cortical control only gradually, at around three to four months of age (McGraw, 1942).

The Differentiation of Behavior. The neonatal behavior so far described may be seen to have in it the beginnings of the whole gamut of behaviors which make up the mental life of the child and adult: the vegetative reflexes, the motor coordinations and skills, the intellectual, *e.g.*, perceptive and adaptive learning, the social and the affective or emotional aspects of his personality. In describing the development of any one of them we start with the same simple undifferentiated behavior. With the development of increasingly complex functions the various types of behavior become increasingly discrete. They may then be more adequately described and treated under separate categories.

In so far as there is any sequence in the order in which the various kinds of function become recognizably different from one another, we may say that there is first the establishment of the reflexive life processes. The development of the senses and of the cortex of the brain permits the development beyond the reflex level, of both motor coordinations, on the one hand, and perceptual and intellectual functions on the other. Along with these, the rapidly growing and strengthening muscles are a necessary part of the developing motor abilities. These motor abilities, in turn, make possible the more adequate adaptive behaviors which are the early evidences of intellectual growth. The developing pattern of emotional reactions is in various ways tied in with the motor, intellectual, and physical stages of maturing.

Reflexes.—At birth the life processes of the infant are carried on by a variety of reflexive actions, or physiological functions which, although more or less continuous with their prenatal actions, are

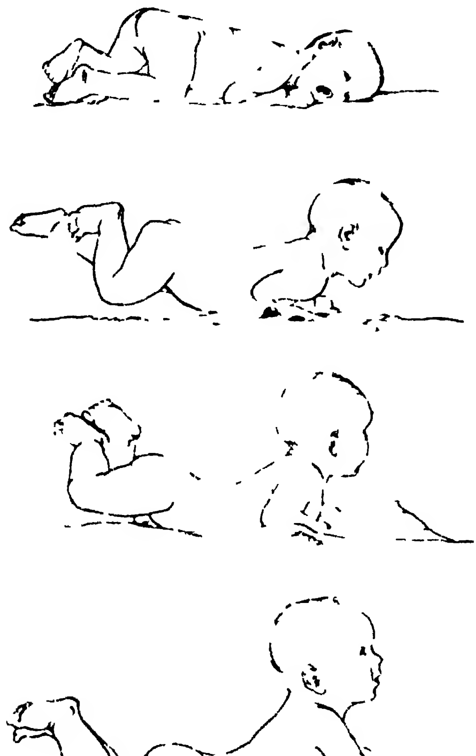
still immature, and in varying stages of development toward their own mature functional patterns. Although the heart started beating in the three-week fetus, its beat at birth is still irregular, and very fast. Even at one month it was found to average 144 beats per minute in the babies of the Berkeley growth study (Bayley, 1940a). The pulse is weak, the blood pressure low. Breathing, which is inaugurated after birth, is fast, shallow and irregular. The sucking and swallowing reflexes are often poorly coordinated. The first few days of life show rapid stabilization of these vegetative functions which will remain, in large part, involuntary and reflex in nature throughout life.

During the first months of life, in addition to the basic vegetative and nurturant reflex activities, there is a rapid growth of the neuromuscular systems, and control of the body (McGraw, 1942). These go through phases of increasing smoothness of reflex function, followed by cortical control. They are evidenced on the purely physical side by rapid growth of the body, of bones, muscles (strength) and fat. The body is beginning to catch up in size with the head, and the infant becomes able, among other things, to support its head.

The postural reflexes, such as head lifting and turning, serve at first to free the face, and especially the nose, from possible suffocation. Later, along with other postural reflexes, they participate in maintaining balance for upright posture and for locomotion.

During this early period several postural reflexes wax and wane. Some, like the grasp-suspension (Pratt, 1946, p. 230) and the Moro (Irwin, 1932), are almost universal during the first month, but drop out quickly thereafter. The Landau is an example of a postural reflex which goes through its complete cycle of development and decline after birth, usually between one and six months of age (cf. Figure 4 2). At birth the infant, when placed prone, tends to curl up in a ball with knees and elbows drawn together under his body. This is the fetal postural position. With development the knees and elbows become less tightly flexed, the head straightens out, and the infant then lies flatly prone. But soon, along with strengthening back, leg, and neck muscles the Landau reflex appears, reversing the fetal tendency. The typical Landau occurs in 90 per cent of five-

month-old infants, when they are placed prone on a hard surface (Bayley, 1940a). The back curves up at both ends, and the head and heels go up, making the line from head to heel approximate



1 month

3 months

5 months

7 months

FIG. 4.2. Age changes in posture of the same child when placed prone on a hard surface. The Landau reflex is fully developed at 5 months.

the curve of a chair rocker, as in Figure 4.2 This reflex does no last long. Its automatic reflex character soon disappears, and by the age of seven to ten months there is evidence of voluntary contro (see Figure 4.2, seven months) The infant will lie flat, or lift his head and push up with his arms without curling up at both ends Soon after this he will pull knees or feet forward into a preliminary

position for creeping. Many of the simpler reflex elements remain, but they are freed from a rigid pattern of action, to permit more varied postural responses.

In general, the simple reflexes are matured soon after birth, or at least by four or five months of age. They are then either superseded by the later-developing voluntary actions or continue to function as basic action-patterns throughout life. These reflexes range from completely involuntary simple actions like the pupillary contraction to light, on the one hand, to constituent parts of learned highly skilled activities such as skiing, on the other.

A survey of the developmental trends so far covered shows that there is a period of early development of reflex behaviors which are neuromotor and under the control of the more primitive nerve centers. The period of rapid development of the reflexes is in large part prenatal, but continues for several months after birth, or until the cerebral cortex attains sufficient maturity for the initiation of voluntary control (McGraw, 1942).

More or less concurrently with the development of reflexes is that of the sense organs, and with them, sensori-motor coordinations. These, along with developing muscular control over the body, form the predominant behavior patterns of the first six months of life.

Motor Development.—The development of motor coordinations depends, in addition to neural growth, on the increasing size and strength of the muscles, and also to some extent on changes in body proportions, and on growth of a more rigid bony structure. The neonate is unable to hold his head up. His muscles are weak and his head is proportionately very large. But at this point the growth of the head has become slow, while growth of the region of the neck and chest is rapid. The neck muscles grow and increase in strength at the same time as the body proportions are changing. Moreover, ossification of the skeleton is continuing rapidly. By the time the infant is two months old he can hold his head erect when he is held upright. Before three months he will usually be able to keep his head from sagging when he is lowered to the horizontal position, preparatory to laying him on his back. And at four months he will

maintain his head in balance while being carried about, tilted or swayed from side to side (Gesell, 1928). Similar examples can be given of this type of rapid growth in strength, in conjunction with further muscular coordinations.

When the Berkeley Growth Study [2] children's test scores in mental [3] and in motor abilities (Bayley, 1933b, 1936) were scaled by Thurstone's (1925) method of absolute scaling the curves of growth

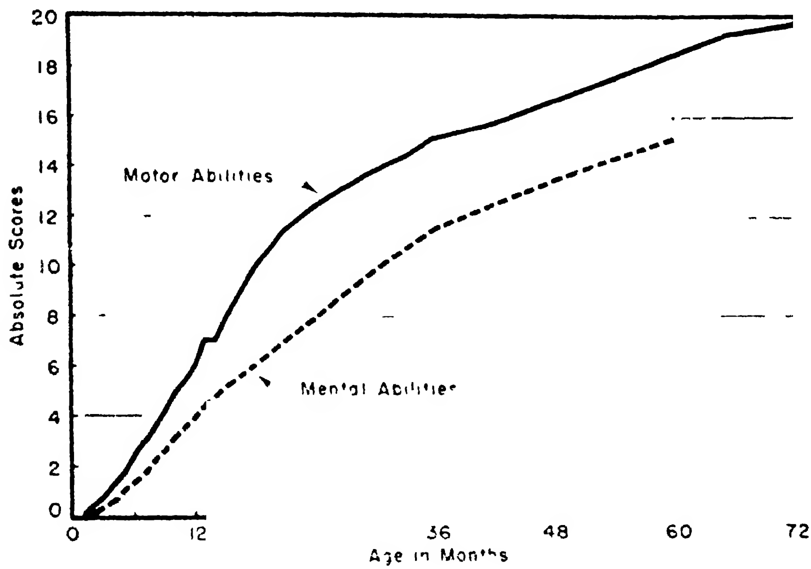


FIG. 4.3. Curves of growth in mental and motor abilities of children in the Berkeley Growth Study according to Thurstone's method of absolute scaling.

for age in both types of behavior were found to be steep at first, leveling off later (Bayley, 1935). As compared with the mental scores, motor scores showed more rapid early growth with a more abrupt slowing down, at around three years of age.[4] Inspection of these two curves, shown in Figure 4.3, leads to the conclusion that

[2] This study of the authors (often abbreviated as B.G.S.) will be referred to frequently in giving examples of different aspects of development.

[3] The nature of mental functions is discussed in a later section.

[4] This difference is based largely on the differences in variability of performance in the two tests. The S.D.'s of Motor Point scores at any given age are smaller than the S.D.'s of Mental scores at the same age. Also the individual items on the Motor test have smaller age-ranges of first passing than do the mental items.

the development of such motor abilities as general bodily control (as evidenced in maintaining balance, and in locomotor activities) mature early, and well in advance of intelligence

TABLE 4.1 —CORRELATION COEFFICIENTS BETWEEN INTELLIGENCE TEST SCORES AND SCORES ON TESTS OF LARGE MUSCLE ABILITIES FOR SUCCESSIVE AGES FROM ONE MONTH TO NINE YEARS FOR THE BERKELEY GROWTH STUDY CHILDREN

With the Infant Scale of Motor Development

Age in Months	N	r	Age in Months	N	r	Age in Months	N	r
1	53	49	8	53	44	15	49	54
2	58	27	9	56	60	18	51	34
3	61	52	10	56	46	21	53	22
4	58	48	11	52	64	24	48	14
5	58	55	12	54	54	27	51	27
6	57	60	13	55	47	30	47	18
7	52	61	14	50	62	36	49	51

With Tests of Jumping and Balance

Jumping			Balance		
Age in Months	N	r	Age in Months	N	r
54	42	22	54	43	22
60	44	04	60	46	−02
72	44	30	72	44	09
84	43	02	84	44	15
96	46	20			
108	42	14			

The mental and motor functions are not at first independent of each other, however As is shown in Table 4.1, the correlations between cumulative point scores on the mental scale and the motor scale are in the neighborhood of .5 for the first fifteen months, after which they drop abruptly to around 2. The relationship is even

lower after five years of age, between intelligence and two groups of motor tests which are characterized as Jumping, and Maintaining Balance

There would appear, then, to be both an interdependence in the development of early mental and motor abilities, and an independence in their rates of maturing It would seem that the adequate functioning of either kind of behavior is dependent on a certain minimal ability in the other During the first year or so these minimal abilities are being established in both, and the child's behavior development is not well differentiated into specifically intellectual (adaptive) or motor acts

In general, all the behavior of the young infant has been seen to have a very large component of motor function (see page 169) The early increase in motor abilities is also seen to be in certain respects dependent on physical growth and development

Strength is an important factor in many motor activities and strength is determined in large part by the size of the muscles, as well as the size proportions and hardness of the bones which support the muscles and give them leverage These physical factors influence the large-muscle activities which are concerned with postures of the body such as gaining and maintaining upright positions, or what we have called antigravity behavior (Bayley, 1935) and locomotion (walking running climbing) and combinations of these, such as jumping throwing and a variety of athletic skills Vestibular sensitivity for maintaining balance is also important for these large-muscle activities

Another, in many ways very different kind of motor activity is found in the manipulatory skills which require speed of action and accuracy in the coordination and movements of small muscle groups, and probably also keen sensory discrimination in tactile and muscle senses, especially in the hands This type of skill was found, for the Berkeley Growth Study, to be somewhat more related to intelligence scores, as may be seen in Table 4 2

To continue with some comparisons of the motor scores for this small sample (whose performance appears to be representative in these respects), we find that the children's scores in Manual Dex-

terity[5] are unrelated to their strength, as measured on the hand dynamometer,[6] starting at eight years of age. The r's for ages 8 through 11.5 are shown in Table 4.3. But Manual Dexterity is moderately related to large muscle skills and the latter are moderately related to strength.

TABLE 4.2. CORRELATIONS BETWEEN SCORES ON THE MANUAL DEXTERITY TESTS AND INTELLIGENCE

Age in Months			
Man. Dex.	Intelligence	N	r
54	54	42	30
66	60	45	44
66	72	56	39
78	72	41	42
78	84	40	39
90	84	45	35
90	96	45	31
102	96	46	19
102	108	44	21
114	108	43	53
114	120	42	53
126	120	46	43
126	132	45	46
138	132	45	40
138	144	41	57

As for growth trends, there are continued increments in the scores for all motor skills into young adulthood (Espenschade, 1940). Strength increases until physical maturity is reached, when it is maximal, at least for girls (Jones, 1949a). In accordance with this condition, development of skill in athletic events requiring strength shows marked sex differences after twelve or thirteen years. The greater musculature and larger bony frame and shoulder breadth give the boy of fifteen or older a great advantage over girls and

[5] A composite of 13 tests: Three-hole Coordination, Bolt-and-nut Assembly and Dissemble, Tapping right and left, Synchrometer, Sorting (best hand), Serial Action, Reaction Time and Speed Drill (Miles Rotor). Described in unpublished manuscript.

[6] Sum of best scores (3 trials each) for right and left grip, pull and thrust.

younger boys in athletic skills. This difference is also found, within each sex, among children of differing degrees of physiological and anatomical maturity.

TABLE 43 INTERRELATIONS AMONG MOTOR TEST SCORES IN THE BERKELEY GROWTH STUDY

A. Correlations of Manual Dexterity with Strength, Jumping and Balance

Manual Dexterity	Strength			Jumping*			Balance†		
At Months	At Months	N	r	At Months	N	[illegible]	At Months	N	r
54				54	4[illegible]	44	54	42	33
66				66	45	55	66	46	19
78				78	41	[illegible]	78	41	26
90	96	44	08	90	45	4[illegible]	90	4[illegible]	.34
102	96	45	1[illegible]	102	44	49			
114	114	4[illegible]	0[illegible]						
126	126	46	14						
138	138	4[illegible]	7						

B. Correlations Between Strength and Balance

Age in Months	N	r
96	45	30
108	42	3[illegible]

C. Correlations Between Jumping and Balance

Age in Months	N	r
[illegible]	41	40
66	44	20
[illegible]	4[illegible]	49
78	41	55
84	4[illegible]	0[illegible]
90	42	− 0[illegible]

* Jumping Scores are the sum of scores on 3 tests: [illegible] jump, high jump, and jump and reach.
† Balance scores are the sum of scores on 5 tests: [illegible] (eyes open, and eyes closed), right foot, and left foot.

Jones (H. E. Jones, 1940a) has shown in studies of growth in strength of adolescents, that increasing S.D.'s in the strength scores are related to the children's differences in rates of physiological ma

turing. The earlier maturers had an advantage in strength at around thirteen to fifteen years which they lost as the slower-growing children gradually caught up. He also found differences in the growth rates for the different tests of strength, i.e., hand grips, thrust, and pull, as well as differences between the sexes.

These same adolescents were given a series of motor tests by Espenschade (1940). She obtained intercorrelations usually about .4 to .5 between scores on such items as broad jump, 50-yard dash, jump-and-reach, distance throw, and the Brace tests of bodily coordination and agility. The target throw, which involves hand-eye coordination, was less closely related to these other large-muscle performances, correlating only about .25 with the other tests. Of the more closely related tests, all but the Brace scores are strongly correlated with strength among the boys. The girls, who were much more nearly mature when studied, showed little relation between their strength and scores on these tests of gross motor performance.

The Brace tests of agility were found to be correlated substantially with two tests of fine motor performance, reaction time and a test of coordination time with the hands. The r's are in the neighborhood of .35. But the other four tests of large-muscle skills were only very slightly related to these tests of manual ability.

From these various comparisons it becomes evident that there is much more independence of function in all motor behavior items than is found for the items used in intelligence tests at the same ages. Intercorrelations between motor items are rarely as high as .5. The higher r's occur only between very similar activities involving the same or contralateral groups of muscles.

Furthermore, there is little consistency betwen scores on tests separated by two or more years for these relatively independent motor activities. This is illustrated in Table 4.4 for the Manual Dexterity scores of the B.G.S. cases between 4.5 and 11.5 years of age. Although these r's are higher than the consistency correlations reported (Bayley, 1935) for the motor test scores of these same children during their first three years, they are still considerably lower than the consistencies of their mental test scores for comparable ages and intervals (Bayley, 1940b).

Various factor analyses of motor test scores usually identify strength, speed, and precision (or coordination) as the principal factors in motor skills. From the size of the correlations obtained between motor skills, generally, it would appear that the functions falling within any one factor are not strongly interrelated, but remain relatively specific. So far, there have been no factor analyses made within the field of motor abilities, which are concerned with age changes in specificity of function during infancy and early childhood. We may assume, provisionally, that many of the motor skills develop functional specificity at about the same time they become independent of the more intellectual kinds of behavior. This would be at about eighteen months if we use as the criterion the dropping correlations between mental and motor scores found for the B.G.S. cases.

TABLE 4.4. CORRELATIONS FOR CONSISTENCY OF SCORES IN MANUAL DEXTERITY. BERKELEY GROWTH STUDY

Age in Months	66		78		90		102		114		126		138	
	N	r	N	r	N	r	N	r	N	r	N	r	N	r
54	42	.54	36	44	39	58	40	.58	38	.56	41	.28	39	.29
66			39	73	43	70	43	63	41	.70	44	.48	42	.50
78					40	78	39	63	37	63	40	53	38	49
90							44	81	42	61	46	46	43	.55
102									43	53	46	36	44	.49
114											43	.71	42	.68
126													45	.85

Early Mental Development: Sensori-motor and Adaptive Behavior.—An important aspect of development in the neonate is a group of activities which have been described as sensori-motor coordinations. Growing, strengthening muscles, maturing neural tracts, and increasing acuity of the sense organs make for more ready responsiveness to the stimulating environment. Their developmental changes are primarily in the direction of more coordinated and better adapted bodily adjustments.

During the second half year, at the time the cerebral cortex is beginning to function, we find the inception of more distinctively

adaptive, or intelligent behavior. At this time the senses are relatively mature, and their development is slower. Among the behaviors which are developing under cortical control, the motor skills seem to have had a start over the intellectual ones, and grow more rapidly for a time. But, as we have seen, after about two years the motor growth slows down, while the growth of intellectual functions continues to be rapid, and the mental and motor functions become relatively independent of each other.

In selecting tests of mental ability for infants under five months of age, there appears to be no choice except among reflexes, some neuromotor coordinations, and some sensory acuities or discriminations and lengthening spans of attention in responding to sensory stimulation. Investigators have varied in the extent to which these different types of behavior have been included in their tests. Linfert and Hierholzer (1928) included a relatively greater number of the more obvious reflexes, such as plantar reactions to stroking the sole of the foot with a feather. Although Gesell includes the large-muscle coordinations of the body in his Developmental Schedules, he classifies them in a separate category which permits independent scoring. (He groups the behavior development items into Motor, Adaptive, Language, and Personal-Social.) Bayley (1935) put all behaviors which seemed to be predominantly of motor coordinations and skills into a separate test of motor abilities. However, inspection of the remaining behaviors which were included in the mental scale showed that for the first four months these items were predominantly sensorimotor in character. There were no behaviors in this early segment of the scale which could be classified as primarily "intellectual" or "adaptive."

It has been questioned whether the behaviors tested in these infant scales can be called intelligence (Irwin, 1942, Anderson, 1939). It is obvious that they are in many respects very different from the behaviors we measure in the intelligence tests of school children and adults. Whatever we call them, however, they are the behaviors which characterize the infant's attentive and adaptive responses and adjustments to his environment. They exhibit consistent trends toward increasingly complex and abstract mental func-

tioning. They show rapid developmental changes with age. With the beginnings of language they are evidenced in the understanding and use of symbols, the obvious form of abstract thinking. Whether these behaviors are called intelligence or not may be considered a matter of definition. The scores earned on tests of "intelligence" or mental ability during the first year or two are not correlated with the intelligence test scores earned by the same children at later ages (Bayley, 1940b, 1950; Goodenough and Maurer, 1942). There are, however, according to the general theory of growth presented here, many possible reasons for this lack of correlation. The processes of differentiation, and the differences in rates of maturing of the various functions which become discriminable, are among these. Anderson (1939) contends that it should be possible, by searching for items in the infant scales which do correlate with later intelligence scores, to find a basic core of stable intelligence which persists from an early age. Maurer (1946) has made an effort in this direction, with some small success, in tests of two-, three- and four-year olds.

Irwin (1942), on the other hand, argues that by their very nature infants cannot, in their immature stage of development, have what we consider to be intelligence. Until we do find some core of stable, predictive behaviors in young infants, or have some other adequate validation of them as intellectual functions, it is probably ill-advised to speak of the infant scales as measuring *intelligence*. The general term *mental* is perhaps better, to differentiate the attentive, adaptive behaviors from those which are more definitely motor coordinations.

The easiest items in the California First Year Mental Scale (Bayley, 1933b), placed at about two weeks of age, are obviously reflexes: postural adjustment to being lifted and held to the shoulder, and lateral head movements when placed in the prone position. There follows a series of tests of reactions which indicate the functioning of the sense organs: momentary regard of the dangling ring, response to a sharp clacking sound, prolonged regard, and eye-following of the ring as it is held or moved about in specified directions, and so on

The first item which was classified as "adaptive" was given an age placement of six weeks. This item, "Social Smile," is smiling in response to the examiner's bending over the infant, smiling, nodding, and talking softly (M. C. Jones, 1926). Such a response depends also on an adequate development of sensory acuity, and is therefore only partially determined by what may be an awareness of an appropriate situation for smiling, i e , a sign associated with pleasant affect.

Items with any indication of intellectual activity other than simple sensori-motor behaviors were scarce until around the seventh month. Those which were placed in the "adaptive" series in the original study (Bayley, 1933 a) include between two and five months: shows anticipatory excitement to being lifted; reaches for dangling ring; shows awareness of the strange situation; makes anticipatory adjustment to lifting; closes on dangling ring; turns head toward a sound; reaches for cube By seven to ten months, we find: exploitive string play; manipulates bell with interest in its details; looks for fallen spoon; pulls string and secures the ring, vocalizes recognition, attends scribbling, cooperates in games, interest in throwing; picks up inverted cup and secures hidden toy. The behaviors described by the earlier of these items may be seen to exhibit some evidence that learning or memory is operative in bringing about a response which is appropriate to the situation Slightly later there is added evidence of voluntary, directed attention or manipulation of materials

The division into Sensori-motor and Adaptive classes is to a considerable extent arbitrary, and there remains a strong sensori motor component in most of the adaptive items of the test through out the first year. This classification was made subjectively by the experimenter who judged that certain behaviors were more adaptive (or "intellectual") than others. It might be altered on further analysis. At the time of the original study (Bayley, 1933 a) several combinations of items were selected and the scores of these sub-tests were correlated with each other and with themselves at different ages. Additional methods of classification should prove fruitful in clarifying the picture of early mental organization Groups of similar

items might be selected by means of a factor analysis or an analysis of variance.

A factor analysis has been made by Richards and Nelson (1939) on the items of the Gesell tests given to eighty infants at six, twelve, and eighteen months of age. This analysis yielded two factors, which they called "alertness" and "motor ability." There was some evidence of greater communality in the tests at six months, with increasing specificity at twelve months. Although the items at eighteen months were again more alike than at twelve months, they remained more independent than the six-month tests. The eighteen-month tests appeared to cover a more restricted area of behavior, indicating a greater selection of the behaviors included in the tests at this age.

The items in the California tests and the Gesell tests for the first year are very similar. Since Richards and Nelson included Gesell's items of gross motor coordinations in their factor analysis, it is evident that at least a large part of their motor ability factor is composed of such items. These authors point out, however, that the motor factor is found in almost all of the items and makes up a part of all infant behavior. Similarly, McNemar (1942) found a motor factor in the early years of the Stanford-Binet. It is possible that the alertness factor includes most of the "sensori-motor," as well as the "adaptive," items in our classification at the early ages.

Although the sensori-motor classification in the California First Year Scale drops out in the second half year, there is evidence that increase in sensory acuity does not stop abruptly at around eight to ten months of age. Tests of hearing and vision indicate that they improve throughout the preschool years, at least. But it would appear that the rapid development to a point which is essential for the early mental and motor adaptations is achieved by this early age and that subsequent changes are relatively slow. They may affect the development of other kinds of ability, later. For example, the ability to learn to read is determined in part by the maturation of the eyes, and their accessory muscles sufficiently to permit discrimination of the forms of letters and words, as well as to make possible continued fixation of the eye on the printed page.

Age Trends in the Growth of Intelligence.—On the basis of a number of studies which have been made of age changes in intelligence, we may construct a theoretical curve such as that presented in Figure 4.4. Although this curve is based on data from only two studies (Bayley, 1950, and Jones and Conrad, 1933), it is typical of many for relevant ages. The first few years of the curve has its counterpart in the studies of Gesell (1946). Varying sections of

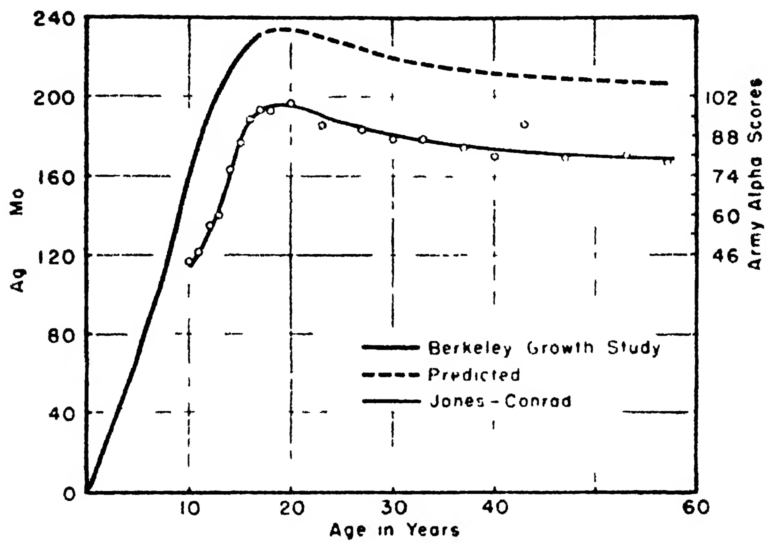


Fig. 4.4 A theoretical curve of intelligence by age, based on two sets of data: Berkeley Growth Study mental ages from one month through 17 years (Bayley, 1950) and Army Alpha scores from 10 years through 60 years (Jones and Conrad, 1933). The two series are adjusted so the curves are parallel at overlapping ages. The B.G.S. curve for later ages is then predicted to parallel the Jones and Conrad curve.

the childhood years are covered by such studies as those of Goodenough and Maurer (1942), Freeman and Flory (1937), Dearborn and Rothney (1941), to mention only a few. The studies of H. E. Jones and Conrad (1933), of Wechsler (1944) and of Raven (1948) and Foulds (1949), follow this pattern for maturity and old age, as well as for the later portions of childhood.

The curve of growth in height, Figure 4.1, has the advantage of being based on units (centimeters) which are known to be equal for all parts of the scale. Although there have been many attempts

to devise "absolute" scales of mental functions, none of them has been entirely successful. The curves presented in Figure 4.3 are based on Thurstone's method in which he assumes a mean and standard deviation for one age, and interprets those of all other ages in the series in their relation to this standard. Thorndike, Freeman, and others have devised more or less similar methods for producing equal units of scaling. They are all based on assumptions which may not necessarily hold for all ages and functions or degrees of difficulty.

The curves in Figure 4.4, then, must be considered as approximations. But they do show the general trend of rise and fall with age. Intelligence increases fairly rapidly to a point around twenty-two years, and then decreases again, somewhat more slowly. The B.G.S. curve is drawn from mean mental ages of that group, and has the advantage of being derived from scores on the same population, tested repeatedly from one month of age. The curve of Army Alpha scores from the Jones and Conrad study (1933) has been plotted in such a way that it parallels the B.G.S. curve for the ages where the two overlap. It is interesting to note that when the B.G.S. curve is extended so that the parallel is continued to age sixty, it gives a curve rather similar to the curve for height. However, it is meant here only to suggest the probable tendency of the average change of intelligence with age.

There is an increasing accumulation of evidence that intelligence grows until the latter part of the second decade and probably into the twenties (*e.g.*, R. L. Thorndike, 1948). Growth slows down, however, after thirteen or fourteen years, continuing slowly until the point of highest average intelligence occurs, near twenty years of age; after this there is a gradual decrement with age in the scores. The early studies on the growth of intelligence seemed to indicate a younger age for achieving mature intelligence. It was estimated that mental growth stopped some time between thirteen and sixteen years. The 1937 Stanford-Binet IQ's (Terman and Merrill, 1937) are adjusted on the assumption that mental ages reach their highest point at sixteen years, and remain stationary thereafter. It would appear from the studies cited here that this is not the case. Mental scores continue to increase for several years, and they do not then

remain stationary. Of course the crucial point on this will be determined when the same persons have been tested repeatedly through at least a portion of their adult span. Freeman and Flory (1937) found that those students who went to college continued to grow in intelligence, according to retests at around twenty-one years. There are as yet no data available on noncollege subjects or for the average or lower IQ levels, for repeated tests after about eighteen years.

The curves in Figure 4.4 represent averages of scores on tests which are made up of several different types of intellectual function. To accord with our second principle, mental functions should become differentiated during the processes of development, in somewhat the same manner as we have demonstrated the early separation of reflex, sensori-motor, motor, and adaptive functions in early infancy. Furthermore they may be expected to have somewhat independent courses of growth and maturation, so that this theoretical curve may be looked upon as a smoothed composite of a group of slightly different curves, none of which follows this one exactly.

There have been several different approaches in the investigation of the nature of the components of intelligence. We are not concerned here with deciding which types of function are to be included in the general category of "intelligence." We are more interested in observing the developmental courses of those intellectual behaviors whose age changes have been studied and compared. Probably the best information for this purpose has been furnished by the factor analyses of intelligence at different ages. These studies have been very well organized and presented by Garrett (1946). He defines intelligence as including "the abilities demanded in the solution of problems which require the comprehension and use of symbols." He examines the factor analyses of Spearman (1927) and of Thurstone (1938) and a series of factorial studies on age changes in mental organization (Anastasi, 1932; Asch, 1936; Clark, 1944; Garrett, Byron and Perl, 1935; Kelley, 1925; and Reichard, 1944). In all of the studies cited by Garrett there is evidence that the factors which have been isolated statistically become more independent of each other with increasing age. Garrett points out that

Spearman, who based his factor analyses primarily on children, found that most of intelligence could be accounted for by one general factor, or *g*, and a series of specific factors. Thurstone made his factor analyses on young adults (college students). He found ten "primary mental abilities," seven of which were clear enough to name. However, when he extended his tests of primary mental abilities to children he found that these abilities were less discrete. He then postulated a second-order general factor. In a series of studies based on Thorndike's CAVD and similar tests, it is shown that the younger the children the higher the intercorrelation between tests. There is a strong indication that intelligence in young children tends to be general, but that its components gradually become less correlated, so that by the "high school and college levels, abstract intelligence breaks down . . . into a number of relatively independent factors." (Garrett, 1946, p. 377) Hsu (1948) has expressed a similar theory of the growth of intelligence as seen from factor analyses. He calls it "differential bio-process."

Several recently reported studies fail to support this thesis. Two of these (Doppelt, 1949; Clark, 1949) show no consistent age trends in mental organization in tests of children six to seventeen years of age. However, it is possible that the functions compared had already reached their maximum independence at the youngest ages measured. It may be expected that some functions become independent at much younger ages than others. In two other studies (Chen and Chow, 1948; Curtis, 1949) there appears to be an actual increase in the communality of the intellectual functions measured during the school ages.

What are the reasons for these apparent contradictions? Or has there been some error running through one group of the experiments? It has become evident, for one thing, that the factors which are derived by the accepted statistical procedures, although they often make sense, are very dependent on the specific contents of the test-batteries which are factored. They are probably also influenced by the nature of the populations tested. College students, for example, are not as representative of the population as grade-school groups. If enough different aspects of behavior can be tested on

adequate populations, we may be able eventually to winnow out those "primary mental abilities" which remain stable.

Within the framework of our theory of development, it is possible that an adequate covering of the development of all types of intellectual functions will reveal that with maturation there will be a certain amount of reintegration of some functions which have for a time been relatively discrete. Abilities which have been developing at different rates may, as they approach mature status, become more closely allied within the individual. On the other hand, the differential rates of decrease in abilities after maturity (see later discussion p. 183) would appear to accentuate the specificity of many intellectual factors.

Garrett's and Hsü's theories of the development of abstract intelligence are in general accord with our second principle, so far as they go. But these theories should be expanded to include processes of reintegration of mental functions. Furthermore they do not take account of our third principle, of differences both in the age at separation and in the growth rates of the different intellectual functions. An adequate test of this principle may require the testing of a much wider range of abilities. There are, however, some data available which bear on this problem.

In the original analysis of the Berkeley Growth Study test material, a striking reversal in the trend of S.D.'s of the scores was revealed (Bayley, 1933 a). Although the means of the cumulative point scores increased rapidly with age, the S.D.'s increased from one to six months, then dropped sharply to twelve months, after which they again increased (Cf. Figure 4.5).

Investigation of the possible causes led us to the conclusion that we were testing two types of function which were developing successively rather than concurrently. In their early development of sensori-motor functions the infants' scores exhibited a normal tendency to become more variable during their period of rapid growth, because of the individual differences in rates of growth. But as these functions approached maturity, between the ages of six and twelve months, the scores became more alike.

If these items had been replaced with an adequate supply of adaptive items during this period, the S.D.'s would probably not

have reversed their trend But apparently there is a relative lag in the development of the more intellectual or "adaptive" kinds of behavior, and no supply of such items was available Consequently, the variability of adaptive items increases slowly, and enough later than the sensori-motor behaviors, to introduce a break in the age trend of S D 's for the total scale This break is more obvious than

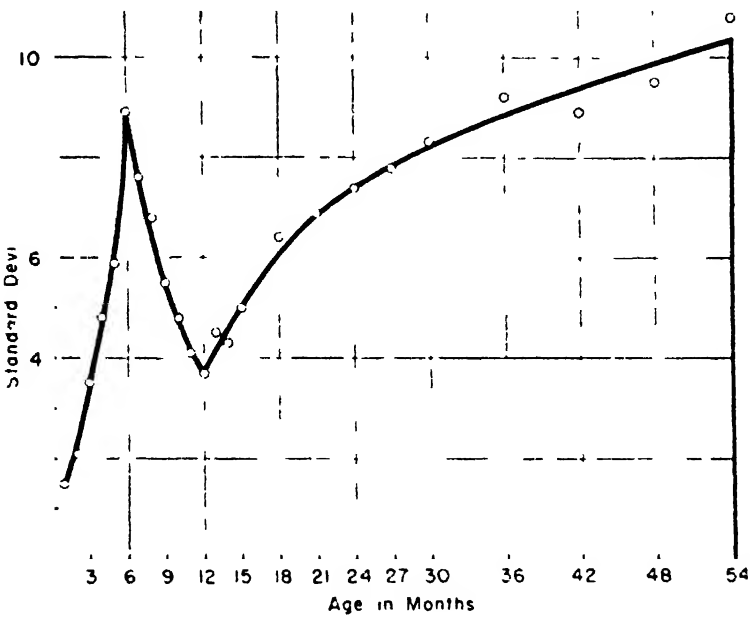


FIG. 45 Age curve of standard deviations of mental test scores in the Berkeley Growth Study (From N Bayley Genet Psychol Monogr 1933 14, No 1, by permission of The Journal Press

at any later age, probably because the later developmental trends of the more complex intellectual abilities of childhood are occurring more or less concurrently, on overlapping and somewhat similar time schedules

Yacorzynski (1949) has recently called attention to the independent growth rates of two mental functions He was interested in showing that learning and perception are different and to a considerable extent independent functions As evidence for this, he cites studies to show that a primitive form of learning has been observed soon after birth and that learning reaches its mature

capacity around twelve or thirteen years. Perception (or "ability to see relationships") has not been noted until the cerebral cortex has started functioning, after three months of age Furthermore, it apparently continues to develop into adult life

Maturation of Intelligence.—The course of development and maturation of a structure or function may be judged by two things, (1) the changing rates of growth or increments in the scores of individuals, or of the mean scores in a group at successive ages, and (2) the age trends of the variability of the scores in a constant population which is retested at regular intervals. If mental functions could be measured in absolute units, it would be easier to determine both of these factors However, various devices have been used to obtain approximately comparable units of ability. Although the growth curves of the different mental functions vary in their rates of change, and in the time required to reach maturity, there is a general similarity in them all As we have indicated in Figure 4.4, they start with a period of rapid early growth, followed by more or less continuous, steady, increments, until maturity is approached and growth gradually ceases

In physical growth, the S D.'s of such growth curves increase in magnitude so long as growth continues to be rapid, but as the mature status is approached and increments decline, the S.D.'s also diminish. This trend in the S D.'s may be attributed to the fact that there are individual differences in rates of development The extremes (the fast and slow maturers) in a given function become increasingly deviant from the average for their age, with increasing age. This dispersion of scores continues until the faster-growing individuals approach maturity and slow down to scores nearer the group mean, while at the same time the slow maturers are continuing at undiminished rates Toward the mature end of the scale the scores for any age-group are composed of some early-maturing individuals who have stopped growing, of average individuals who are continuing to grow slowly, and of the laggards who are by now rapidly catching up with the others because their growth rates have not yet slackened.

The extent to which the standard deviations reduce will be determined by the nature of the variability in a given function (and the method of scoring it) in mature individuals. The S.D. may actually approach zero if "maturity" is defined as a given stage in the life process. This is true, for example, in skeletal age, as rated on the Todd (1937) or similar standards, in which maturity is defined as closure of the epiphyses of certain long bones. With rare exceptions, the epiphyses in these bones close at about the time when growth in height ceases. Todd assigned a skeletal age of eighteen years nine months to the time of closure of the last epiphysis in boys' hands: therefore, all young men (except for a few deviants such as ateliotic dwarfs) over twenty-three years of age will have a Sk.A. of 18.75, and the S.D. (of any discernible differences in maturity) will be zero. This is so, even though the S.D. in skeletal ages was approximately one year at the age of fifteen.

Most measurable structures and functions, however, are not identical, but are distributed more or less normally, in their mature state. We may assume that this is so, regardless of the extent to which these differences are innate or induced by environmental differences. It seems reasonable, therefore, to expect that these individual differences would continue approximately constant, or else increase at a constant rate, throughout the period of growth, if all individuals matured at the same speed. The effect on variability of differences in rates of maturing can be demonstrated for growth in physical size, where we have absolute units of measurement. It is possible to reduce, considerably, the S.D.'s in measures of size during the period of adolescent growth, by classifying the children according to a maturity indicator such as Sk.A. instead of by C.A. So long as measures of size are grouped by C.A., the S.D.'s increase during growth, and diminish somewhat as growth ceases.

Reasoning by analogy from the physical data in which growth units are known to be equal, we may then postulate similar forces operating in the pattern of expanding and contracting S.D.'s of the scores on any behavioral functions whose means show rapid growth followed by cessation.

The data of the Berkeley Growth Study show this to be the case for the sensori-motor functions measured in the Mental Scale for the

first six months, and for the motor coordinations measured by the Scale of Motor Development, for the first thirteen months (Bayley, 1935). After these ages, both scales appear to measure functions different from those measured at earlier ages. Similar reductions

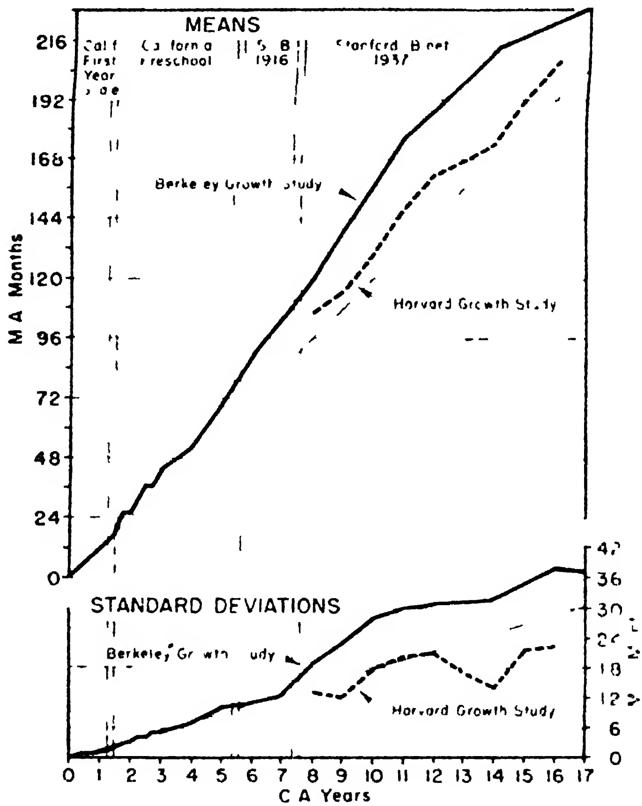


FIG. 46. Means and S.D.'s of mental ages of the Berkeley Growth Study cases from one month through 17 years. (From N. Bayley, *J. Genet. Psychol.*, 1949, 75, 165-196, by permission of The Journal Press.)

in variability of IQ or point scores are to be found at or near one year of age for the Kuhlmann-Binet (Kuhlmann, 1939), the Iowa Infant Scale (Fillmore, 1936), and the Gesell Scale, as used in the Fels Foundation growth studies (Nelson and Richards, 1940).

There is also evidence for individual differences in rates of mental growth during childhood. The intelligence test scores for the

B.G.S. children repeat the pattern of increasing and decreasing S.D.'s at later ages, as may be seen in Figure 4.6. The curve of mean mental ages indicates a diminished rate of growth after fourteen years, and the S.D.'s decrease after twelve years. The S.D.'s of the IQ's (Figure 4.7) exhibit very marked trends which would indicate the phenomenon of approaching maturity in the intellectual functions measured by the tests used, after ten or eleven years of age.

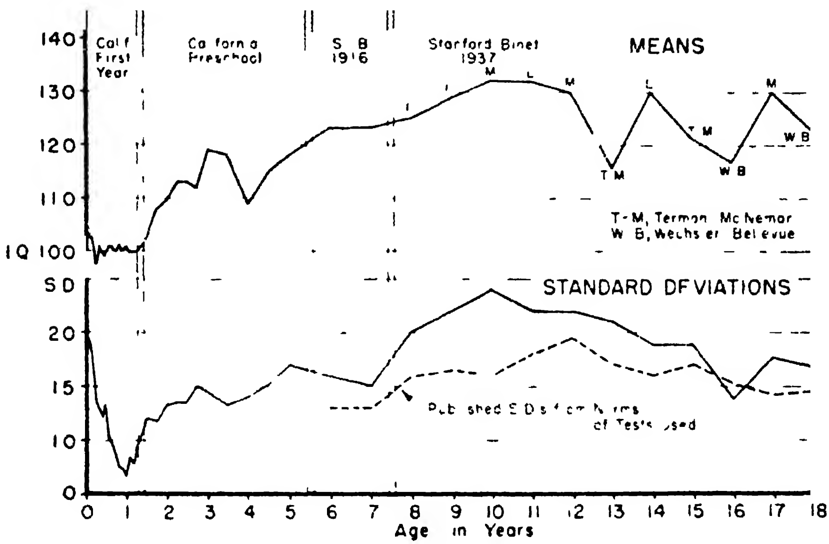


Fig. 4.7. Means and S.D.'s of IQs of the Berkeley Growth Study cases from one month through 18 years. (From N. Bayley, *J. Genet. Psychol.*, 1949, 75, 165-196, by permission of The Journal Press.)

Goodenough (1942) called attention to a similar trend in S.D.'s for the Stanford-Binet, 1937 standardization sample, and suggested that this was a function of the test, which should be corrected. It is true that such a correction is important for achieving more stable IQ's in children making high or low scores.

However, these age differences in variability may not be an artifact of the scale. It is our hypothesis (Bayley, 1950) that these trends in variability are inherent in the processes of mental growth. They should be expected in any adequate test of increasing abilities unless the scores are expressed as indices of relative deviation (*e.g.*,

standard scores) from successive age norms. It is interesting to note that the same trends in S.D. are present in both forms L and M of the Stanford-Binet scale. Their S.D.'s are high at year 2.5, decrease steadily to a low at year six, then increase again to a high at year twelve, after which they become gradually restricted.[7]

Terman and Merrill have suggested that the low S.D. at age six might be accounted for by sampling error, or "an artifact of the nature of the scale." They also conjecture that the high S.D. at age twelve may be related to "the differential age at onset of pubescence, although it has yet to be demonstrated that pubescence is significantly related to the rate of mental growth" (1937, pp. 40-41). Although a number of studies (*e.g.*, Abernethy, 1936, Stone and Barker, 1937) have shown some relation between IQ and rates of physical maturing, the relationships are very slight, and the weight of evidence is that these two types of function are largely independent in their rates of development (Simmons, 1944, p. 82). There is, furthermore, no evidence that the mental functions tested in the Stanford-Binet mature earlier in girls, even though girls' pubescence averages two years earlier than that of boys. It would seem more probable, then, that these two types of function (one mental and one physical) just happen to be maturing at about the same time and thus expressing similar trends in variability.

The age changes in variability of the 1937 Stanford-Binet are present in a constant sample of the Berkeley Growth Study cases who were given this test at seven ages, from eight through seventeen years of age (Bayley, 1950). For this age span the standard deviations of their scores follow the same pattern as the standardization sample. Therefore, the changes cannot be accounted for by differences in the selection of the sample at different ages. As for the artifacts of the test, the Stanford-Binet is not alone in exhibiting this phenomenon.

Age changes in variability of scores between eight and seventeen years as reported by Freeman and Flory (1937) for the VACO (Vocabulary, Completions, Analogies, Opposites) Tests are very

[7] Terman and Merrill are aware that the extremes at six and twelve years are greater than chance. McNemar (1942) gives corrections for adjusting the IQ's at ages where the S.D.'s deviate significantly from 16 IQ points.

similar to those found for the Stanford-Binet for the same age interval. Freeman and Flory were unable to eliminate the trends by using constant samples of children who had been tested repeatedly for periods of seven years. They also were unable to increase the variability of scores at the upper ages by adding "top" in the form of a greater number of difficult items: the children simply failed most of the difficult items, and maintained relatively unchanged scores.[8]

Freeman and Flory's study of the VACO tests is of further interest when the four components of the test are treated separately. Each has a different trend in the growth curves of the means. For purposes of comparing their relative rates of growth, the mean scores at each age, for the four tests, have been converted into deviations from their seventeen-year mean in terms of the seventeen-year S.D.'s. These are shown in Figure 48. According to these curves, which are similar in shape to the raw score curves of Freeman and Flory (1937, pp. 44-45), the Analogies test has shown most gain during this nine-year period. Second in gain is the Opposites test. Also both of these tests approached their mature (seventeen-year) status earlier. The slower-growing Vocabulary and Completions tests, on the other hand, give evidence that growth will continue after seventeen years. These differences are corroborated in the growth curves of the 26 cases who were tested later while in college (Freeman and Flory, 1937, p. 82). The four sub-tests also have different periods of greatest variability. The early-maturing Analogies and Opposites had their periods of greatest variability at twelve and thirteen years. The still-developing Vocabulary and Completions tests maintain only slightly diminished S.D.'s at seventeen years (Freeman and Flory, 1937, p. 38).

It seems reasonable to argue from such data that the general trend of mental development during childhood and adolescence, as

[8] Studies which report repeated tests on the feeble minded indicate that such children make progressively lower IQ's with advancing age and that they do not show any tendency to catch up later. Apparently extreme deviates, with very low potentialities for growth do not fall into the same pattern as the slow-maturing but more nearly normal individuals. The feeble minded may be analogous to the ateliotic dwarfs who never attain normal height although they remain skeletally immature.

measured by our current tests of intelligence, is actually a composite of a series of growth trends in mental functions which are more or less independent of each other. This independence may be found in their rates of growth and in their times of maturing, as well as in the nature of their operations as mature functions.

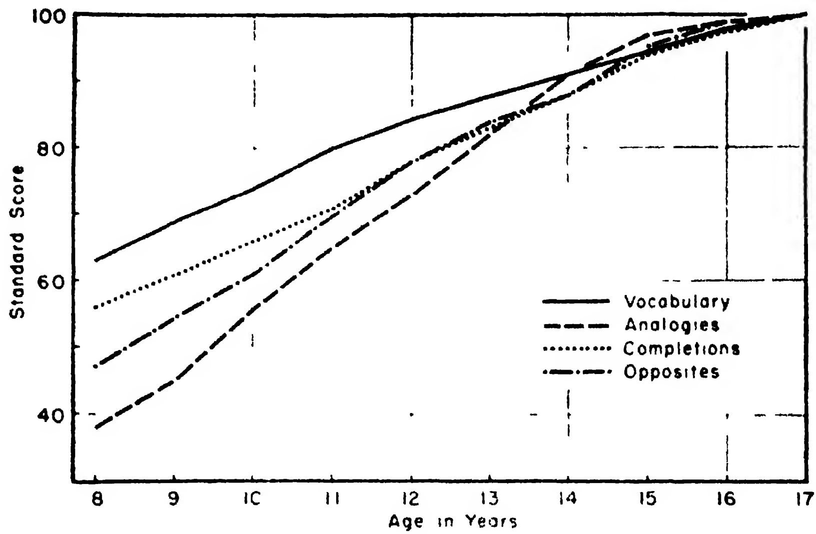


FIG. 48 Comparative curves showing differing rates of growth in four tests of intelligence administered to the same children from 8 through 17 years. Scores have been converted into the number of 17 year S.D.'s from the 17-year mean. (Based on F. N. Freeman and C. D. Flory, *Monogr. Soc. Res. Child Develpm.*, 1937, 2, No. 2, by permission of the authors and publishers.)

Age Trends of Intelligence in Adults. In accord with these assumptions are the findings of researches on the age trends of different mental abilities in maturity and old age (Jones and Kaplan, 1945). There is general agreement that most mental functions decline with age after attaining their highest scores in the early twenties. There are differences among the functions, however, in the times of onset, and the rates of decline. Jones and Conrad (1933), for example, found differences in the sub-tests of the Army Alpha, which they gave to adults in a small New England town. Scores on vocabulary and language tests remained fairly high to an advanced age, while the other kinds of tests showed varying degrees of loss in ability, some of which start soon after the function

is matured Another recent example is afforded in the studies made in England by Raven (1948) and Foulds (1949). They gave the nonverbal Progressive Matrices Test and the Mill Hill Vocabulary Test to large numbers of children and men, covering a wide age range The highest scores on the Matrices test were earned by

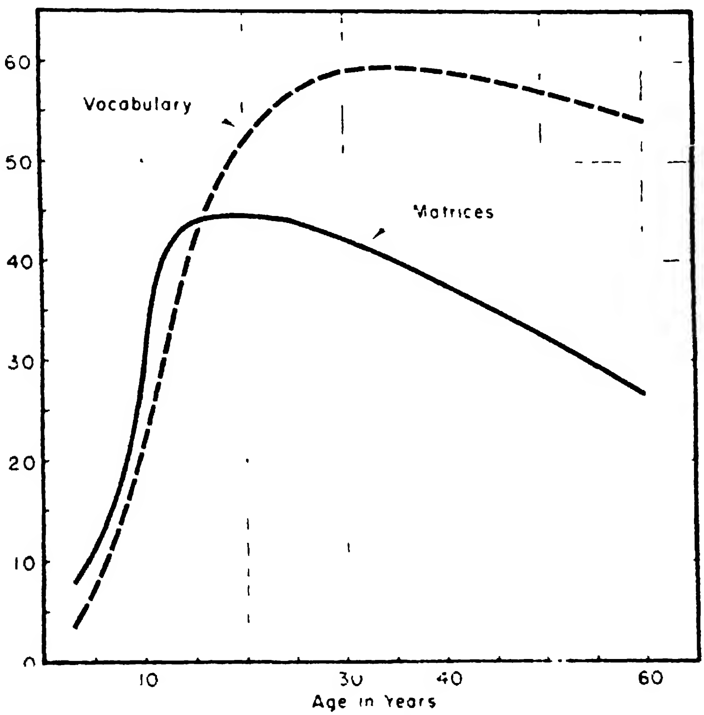


FIG. 49 Age curves for tests of verbal and nonverbal intelligence This is not a longitudinal study, but the same populations took both tests (Adapted from J. C. Raven, *Brit J Psychol* 1948, 39 12 19, by permission of the author and the Editor of the British Journal of Psychology)

sixteen-year-olds, while scores on the vocabulary test increased to thirty years (Cf Figure 49)

It has been questioned whether the decrements with age are due to successive selections of sampling from the less adequate segments of the population Such a question can only be answered finally when the same individuals have been tested at intervals, as they grow from mature adults into old age. However, these findings

occur repeatedly in studies where there has been a definite effort to control the population studied Furthermore, there was much greater decrement in some functions than in others, when a variety of tests were given to the same persons. It seems safe to conclude, then, that there are differences among mental functions in their tendency toward decrements with advancing age. There are probably also individual differences in the retention of once-attained ability in any given function, but, again, to determine this point it will be necessary to make retests of the same individuals after a lapse of years There are other interesting problems which can be raised here concerning the causes of intellectual deterioration. The use or disuse of a function with progressive forgetting, physical restrictions, such as failing sensory acuities or slowing reaction time, are among the causes which might operate in varying degrees on the different functions.

The Effects of Environmental Factors on Development. - Throughout this discussion it has been assumed that variables in the environment affect the course of an organism's development. There has been considerable controversy on this subject, especially as it affects intelligence Many studies have been made, often in attempts to prove or disprove the effects of specific environmental conditions on intelligence test scores These have been treated in great detail in the 27th and 39th Yearbooks of the National Society for the Study of Education (1928, 1940), as well as in many other publications. Researches range from controlled observations of the development of drugged amblystoma (Carmichael, 1926), through co-twin control experiments (Gesell and Thompson, 1929), studies of twins reared apart (Newman, Freeman and Holzinger, 1937) and of the relations of foster children to their true parents and their foster parents (reviewed by Loevinger, 1940).

The general results of the many and varied studies would indicate that the importance of the environment depends on several conditions, among them, the age of the child, the nature of the mental function under consideration, and the nature of the environment.

Studies of Gesell (Gesell and Thompson, 1929) and of McGraw (1935), where one member of a twin-pair has been practiced in

some function, have shown that specific learning experiences are of little avail until the infant is mature enough to profit by them. In children ten to twenty-four months of age a few weeks or months increase in age can make a great difference in their ability to profit from practice. Dennis (1940) found that Indian babies who were kept on a cradle board during most of their first six to eight months were not appreciably retarded in walking. It would appear that in infra-human species, *e.g.*, amblystoma and birds, as well as in the very young humans, the inherent developmental processes are strong, and the opportunity to practice is relatively unimportant, unless restricted beyond some critical point (McGraw, 1946).

But as the children grow older there is much more evidence of the effects of learning on many functions. When sufficiently mature, the child normally learns to speak the language he hears. During the second year of life there is evidence of considerable amounts of learning, not only of language but also of many other things.

The socio-economic variable has been the basis for many comparisons of differences in mental ability. Age changes in these factors have been studied in the B.G.S. children (Bayley and Jones, 1937; Bayley, 1940). It was found that there was no relation between the children's mental scores and their parents' education, occupation, or income during the first year, except for slight negative correlations around five months of age. After eighteen months the correlations became positive. By two years of age mental scores correlated about .5 with parents' education, and the *r*'s remained unchanged thereafter. Honzik's (1940) study on the Berkeley Guidance Study sample corroborates this age trend in relationship.

There is a question whether this increasing relationship is brought about by the effects of environment. It seems very likely that the differences in the nature of mental performance of infants and of adults, as well as differences in growth rates, could account, at least in part, for the absence of parent child relationships in infancy. Two different sets of data are offered in support of this idea. Skodak and Skeels (1949) found zero correlations between IQ's of infants who had been reared in foster homes and the IQ's of their true mothers. However, by the time the children averaged

thirteen years of age the *r*'s with the mother's IQ's were .44. The resemblance had appeared as the children approached the ages at which the mothers had been tested, even though their IQ's were consistently much higher than the IQ's of their true mothers.

TABLE 4.5.—AGE CHANGES IN CORRELATION BETWEEN CHILDREN AND PARENTS IN HEIGHT AND WEIGHT, BERKELEY GROWTH STUDY

Berkeley Growth Study
(Numbers range from 20 to 31)

Age in Months	Boys with Fathers		Boys with Mothers		Girls with Fathers		Girls with Mothers	
	Height	Weight	Height	Weight	Height	Weight	Height	Weight
1	.30	—.05	.05	.35	36	20	30	.41
6	.25	.13	.27	.23	.42	-16	47	23
12	.30	.19	.31	20	36	12	67	35
24	.28	26	.21	02	34	01	56	43
36	.45	.33	25	07	.38	24	67	47
48	.52	.23	.25	12	35	19	66	.41
60	.52	34	30	09	24	37	70	47
72	.52	32	35	.23	26	30	.75	.57
84	53	.23	.35	.20	.33	45	76	62
96	.54	29	.43	.10	26	40	.74	.67
108	.56	28	.43	06	.30	40	.71	68
120	.60	.31	.37	- 01	24	30	72	74
132	.57	.23	.41	07	29	32	68	71
144	53	.30	41	01	24	20	74	.73

There is also some evidence that parent-child relationships increase with age in physical characteristics. An example from the B.G.S. sample is given in Table 4.5 for height and weight. Correlations between the heights and weights of the children and their parents, as reported at the time the children were born, have been computed for a series of ages from birth to twelve years. In this small sample, wherein there appear to be sex differences in the degree of relationship, the findings are necessarily only suggestive. But there is a strong tendency for the *r*'s to increase as the children grow older, from zero to values as high as .6 or .7 in some of the comparisons. We are much more ready to attribute physical resem-

blances to hereditary factors, and to accept the increasing relationship indicated here as unlikely to be due to environmental influences.

Extreme environmental deprivation, such as is found in old-fashioned orphanages or in isolated cases of parental neglect, interferes with mental development. It is difficult to know how much of this failure in mental growth is due to lack of opportunity, and how much is the result of emotional deprivations (Ribble, 1944).

The Development of Emotions. The whole field of emotional development is a very complex one The emotions themselves go through characteristic stages of development and maturation. But, what is also very important, the developments of other mental functions are strongly influenced by emotions. A brief review of emotional development, and some of its relations to the growth of other mental functions, may serve to indicate some of the more salient aspects of emotions in their relation to development and maturation.

If a newborn infant is lying quietly relaxed, a touch or a slight jarring of the bed on which he lies, or a sudden sharp sound, will cause him to become tense. His whole body becomes rigid, or tonic. The reaction, if a strong one, usually also involves characteristic movements of the arms and legs, which extend and then come together as if to clasp something. This is known as the Moro reflex. If no further stimulus is applied, and the infant is allowed to lie quietly, he will relax again. After the first few months the Moro reflex disappears, but a milder form of tensing to sudden stimuli, the startle reflex, remains throughout life.

Tensions and relaxations of the muscles are also induced by internal stimuli. The neonate's rhythms of activity and quiescence which are related to hunger, nursing and satiation, range from extreme relaxation in sleep, on the one hand, to squirming, thrashing about and crying, on the other. Heightened activity and crying may also be caused by painful stimuli, such as pin pricks, heat, or colic pain.

These are the early manifestations of emotional behavior. The generally tonic behavior has been called *excitement;* the extreme

active crying behavior has been interpreted as *distress;* and the calm after-nursing quiescence has been interpreted as a pleasant feeling of well-being or satisfaction. It is from these general gradations in tension and relaxation that the more varied and specific emotions develop.

There are two sides to the process of emotional development. On the one hand there are the emotional reaction-tendencies which become recognizable as expressions of different emotions. On the other hand there are the changing environmental conditions, or stimuli, which cause the emotional reactions. Both of these are related, especially at first, to the development of intelligence and to some extent to motor co-ordinations and skills. However, learning plays a very large part in the emotional changes which take place during growth.

Excitement in the young infant may be positively or negatively toned, depending in part on its intensity, and in part on its duration. Bridges (1931) has named *distress* and *delight* as the first emotions to be differentiated from the primary emotion, or *excitement.* These emotions she describes as developing during the first three months of life. From distress there soon emerge *fear* and *anger* as additional emotions, and these are followed sometime during the first two years by *disgust* and *jealousy.* On the pleasant side, during the first two years, *delight* is further differentiated into *joy, affection* and *elation.* From these, gradually, the whole gamut of emotions develops as the child grows and develops generally.

Basically, that which causes an emotional reaction is some disturbance in the smooth functioning of the individual. In the neonate this is reduced to its simplest terms, in the reflex tensing to a sudden stimulus. The organism's tendency to maintain equilibrium of function, or homeostasis, has been thrown off balance, and it becomes tense and restless until equilibrium is restored. Such disturbances may be pleasantly or unpleasantly toned. They may relate to immediate conditions of the body, as hunger or pain, or they may be related to very abstract associations, such as success or failure in the solution of a problem in higher mathematics. The more abstract the associations, the higher the level of intelligence, or

degree of development, is involved. The pleasant emotions are associated with success in such things as securing food or solving a problem. The unpleasant emotions are associated with failure to solve problems or difficulty in maintaining the sought-after equilibrium.

Obviously, then, the causes of an infant's or child's emotions, and the manner in which he expresses them, are in part functions of his degree of mental and motor development. The neonate's cries are in response to direct discomforts. Because the crying brings the attention of an adult, and relief, this behavior is for a time continued and serves the purpose of keeping the infant relatively comfortable. By two months of age the infant begins to react to people by smiling in response to social situations (M. C. Jones, 1926). Except for the early reflex smile of colic the infant reserves his smile for the social stimulus, and even to the general gestalt of the full-face nodding head which appears before him (Spitz, 1946). This smiling response is associated with many occasions of bodily comfort and pleasant social interaction.

However, around five months of age the child discriminates more details in his environment. He becomes able to tell apart the members of his household. At this time an outsider, a stranger, is recognized as such and is viewed with caution, or even alarm. Many children this age are afraid of strangers, or of being in strange houses, until they have become accustomed to experiences with a greater variety of persons and places (Bayley, 1932; Jones and Burks, 1936).

In general, those disturbing things for which we have no ready response are emotion arousing. When the child has learned adequate ways of responding to new people, he ceases to be afraid of them. When he has learned how to open the gate (or climb the fence) to gain his freedom, he will no longer be angry or frustrated at being penned in. And also, of course, the thrill of a new toy or adventure wears off and may lose interest as it becomes a familiar, everyday experience.

Intellectual development and increasing motor skills afford a succession of newly recognized possibilities for adventure or alarm.

and their subsequent mastery and acceptance. The six-month-old who is left for the first time in the care of a strange nurse, the six-year-old who is sent alone to his first day at school, the eight-year-old who takes his first trip alone on a street car, the young man who makes his first public address, may all find these experiences traumatizing. But if each of these new situations is met with some adequacy of response its repetition becomes less painful, and will often become pleasant, or sometimes even boring.

Again, there are individual differences in the nature and intensity of the emotional reactions to such experiences. These differences appear to be in part inherent: some people are naturally more phlegmatic, others more excitable than the average. But the attitude toward new situations can also be influenced by specific preparation for an event, or general training which will heighten or reduce the intensity of the emotional reaction. McGraw (1939) found, for example, in comparing the later effects of training in motor skills during infancy, that the principal difference between the trained twin, Johnny, and the untrained twin, Jimmy, was one of emotional attitude. Johnny entered into all of the motor activities with great self-confidence and assurance, while Jimmy was timid and cautious in approaching the tasks.

The drives and motivations which make situations emotionally potent are also determining factors in the development of emotions. As we have already noted, the infant's life is at first dominated by the rhythms of hunger, nursing, and elimination. With the growing awareness of the immediate environment the social relationship with the adult who cares for him and feeds him gains importance, and this spreads to other persons.

The pre-school child's emotions revolve primarily around his relations to his family. He may be jealous of a sibling, resistant to adult authority which tries to enforce the cultural tabus and conventions. He is equally delighted and happy in play with, and attention from, the members of his family.

As the horizon broadens, and the child has companionship with his peers, in the school and the playground, many of his drives become attached to these social activities. He feels the need for accept-

ance by his playmates, and he feels rivalry with them in striving for common goals.

By the age of adolescence, this concern with one's age-peers is intensified by several factors in development. With their growing intelligence and experience the teen-agers are learning that their parents are neither omniscient nor omnipotent. They begin to make evaluations and judgments which may be based on attitudes expressed by their friends or independent reading, or other outside sources. These evaluations are often contrary to the teaching and attitudes of their parents. In this stage there is a characteristic revolt from parental authority and an intensification of the need to identify with young people one's own age. The emotionally-charged situations at this age are most likely to center around the conflicts with parental authority and the need to be like and to do what is popular with the others in one's chosen group. Physiological maturation at this stage brings in another set of drives which become strongly potent for emotional behavior. Concern with one's own changing physical condition and one's relations to persons of the opposite sex become important emotion-arousing factors (NSSE Yearbook, 1944; M. C. Jones and Bayley, 1950).

With further advance in age the emotions will in turn revolve around the problems of finding (or not finding) a marital partner, marital relations, the relations with one's own children, and family adjustments, on the one hand, and on the satisfactions, competitions, and strains of functioning as an adequate adult earning a living and striving toward social and cultural goals on the other hand. To a certain extent, many of the earlier situations remain more or less emotionally potent throughout life.

Usually, when we speak of the emotional maturity of a person we refer to the way in which he expresses or conceals his emotions to this succession of emotionally potent situations. But we also refer to the extent to which a person has outgrown the tendency to react emotionally to things which are typically potent only for younger persons.

There is no guarantee that the relatively infantile reactions will be outgrown as increasing intelligence makes possible more mature

reactions. Although, in general, children cease to react emotionally to situations with which they have learned to cope, and develop more mature expressions of the emotions which they feel, there are exceptions. With extreme emotion, and the failure of the more mature efforts at adaptation, there is a general tendency among all people to regress to more infantile patterns of behavior (Barker, Dembo and Lewin, 1941). Some people tend to cling to the more childish emotions, deriving satisfactions from their roles of dependence and the protective care which they are able to elicit from others. Such immaturity can be a form of serious maladjustment.

There is still another aspect to the development of the expression of the emotions. The infant's reactions are typically overt, on the surface. He cries lustily, squirms, kicks, thrashes about. But when his distress is relieved the emotion is immediately over, and he is very soon likely to be smiling, cooing, and laughing happily H. E. Jones (1950) has shown that these reactions are usually on the surface. He has found, for example, that the galvanic skin reflex can rarely be elicited in young infants who are giving strong evidences of emotional behavior. Although there are individual differences in the tendency to internalize the emotions, this tendency becomes much more frequent with age. As the child learns not to cry or to kick and scream, and as he learns to cover up his emotions or to express them only in socially acceptable ways, such signs of internal physiological disturbances as the galvanic skin reflex can be elicited much more consistently in response to emotionally potent stimuli. There is, under these conditions, a tendency for emotion to persist. When the physiology of the organism is affected as shown by the changes in blood pressure, pulse rate, secretion of adrenalin, and so on, the experience of the quality of the emotion lingers, and rapid mood swings are made difficult. Emotional situations also tend to be prolonged with the development of intelligence, as the child is able to anticipate the approach of pleasant or unpleasant stimuli and thus become aroused emotionally before the original stimulus appears. The child cries at the sight of the doctor who hurts him, or shouts with glee at the approach of the uncle who plays games with him.

From this rapid survey of the emotions, it is clear that the development of the emotions in many ways runs a different course from other aspects of physical and mental development. There is, in emotions, very little evidence for independent differentiation and maturation. Rather, the underlying drives and needs which cause emotions, as well as the ways in which they are manifested, are determined by the course of mental and physical development. Within the limits of the organism's intellectual maturity, the specific emotional reactions are highly subject to conditioning, so that the "maturity" of a person's emotional behavior may be largely a matter of the life situation in which he has matured.

On the physical side, growth in size and strength will alter a child's ability to cope successfully with many situations. Furthermore, the physiological and glandular changes of puberty produce a new set of strongly emotional drives. The adolescent's behavior under the influence of these drives will be affected by his intelligence, his motor skills, and his previous learning in regard to the social and cultural mores. But it will also be expressed in the more or less mature patterns of emotional behavior which have become habitual with him during the course of his development (H. E. Jones, 1949b).

Other aspects of development are not independent of the emotions. Favorable and unfavorable emotional attitudes may have profound effects on the development of mental abilities and motor skills, as well as of physical health. For example, intelligence test scores have been found to fluctuate, in many children, in accord with ratings of their emotional or "optimal" attitudes during the test (Bayley, 1940c), and with disturbances in home and family relationships (Honzik, Macfarlane and Allen, 1949). In both of these studies there is evidence for individual differences among children, in the effect of emotional disturbances on intellectual functioning.

Currently there is great interest in the possible influences on all aspects of development, of the emotional atmosphere in which the very young infant is reared. Parental rejection, the frustration of infants' needs for food and for cuddling, and such practices as

bottle feeding and rigid timing of feeding schedules, are considered to have very detrimental effects on the developing personality, and even to interfere with normal mental and physical growth (*e.g.*, Ribble, 1944). However, the researches in this field still leave much to be desired (Orlansky, 1949).

Some psychoanalysts place great emphasis on the effect on personality and emotional adjustments of experiences in early infancy. It is conceivable that the subjection of infants under four months of age to certain kinds of environmental situations could have lasting emotional effects. But it must be remembered that the immaturity of the infant will limit his awareness of these stimuli, and the significance they can have for him. There is probably little more than general distress, at first. If such distress is long continued or recurrent and relatively severe, it could very well be the basis for vague anxieties. There is so far no adequate research to determine the importance of such early experiences for personality formation in an unselected or normal population. It is quite possible that different emotional disturbances are formed at different ages or degrees of maturity, according to the child's capacity to comprehend the disturbing aspects of emotionally-potent environments.

The nature and extent of the effect of different emotion-arousing environments on different aspects of development must be explored much more thoroughly than has been done so far. It is well known that strong emotions such as fear, anxiety, insecurity, rage, and frustration often disrupt normal functions, both mental and physical. We are gradually accumulating more adequate and specific knowledge about the ways in which these emotions may interfere with learning and intellectual growth, and the healthy functioning of the body. It has become a truism that the native mental capacities of an individual should be fostered in an atmosphere of emotional security, but there may be such a thing as too much security. We do not yet know the optimal conditions for development and maturation.

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CHAPTER 5

MOTIVATION

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Introduction

The experimental psychology of motivation, a product largely of the last twenty-five years, developed under pressures from practical interests represented by psychiatry, psychoanalysis, and the social sciences. These diverse influences have visibly contributed to the multiplicity of its concepts and the formlessness of its character. Theory in this field is fragmentary and frequently deficient in generality. At the same time, a strong desire for rapid progress lends acceptance to technically faulty work that would not pass muster in better developed fields. Further, a wide gap lies between some of the most general and stimulating theoretical contributions, which have come from clinical studies of human beings, and much of the best-controlled experimental work, which has used animal subjects. Bridging this gap is an object of much attention at the present time.

Historically Important Theories.[1]—*Instinct Theories.* Most writers on motivation from Plato and Aristotle to Hobbes, Descartes, Spinoza, and later thinkers, have attributed to the human being certain inborn impulses, passions, or desires. Darwin's introduction of the doctrine of organic evolution in 1859 suggested that, if instincts are found in animals, they ought to be found also in human beings. They were, indeed, discovered with little difficulty and in large numbers, since severe criteria were not soon employed. A familiar list of such human instincts appeared in James's *Principles of Psychology* (1890), including, among others, imitation, emula-

[1] See Troland (1928) for an extensive treatment of the history of theories of motivation.

tion, pugnacity, fear, the hunting instinct, acquisitiveness, play, and love.

Machine Theories.—Many thinkers have hoped to explain behavior by physical principles, whether by the motion of atoms (Democritus), the vibrations of "vibratiuncles" (Hartley), or the flow of "animal spirits" through the nerves from sense organ to brain to muscle (Descartes—who, however, exempted the human being from the complete determinism of his theory by permitting the soul to act upon bodily processes). Later theories of tropisms (Loeb), conditioning (Pavlov, Bechterev), and stimulus-response connections (Watson, Thorndike, Hull), exhibit a similar hope of understanding the behavior of organisms on principles similar to those that apply to man-made machines.[2] The machine theories are not so much theories of motivation as attempts to account for behavior *without* the concept of motive; or, if this concept is employed, the theorist is likely to define it as a particular kind of *stimulus* (cf. Miller & Dollard, 1941, and the discussion of Guthrie in the section on The Nature of Motivation and Its Role in Theoretical Systems).

Hedonistic Theories.—A persistent theme running through the psychological writing of all periods is the description of behavior as the pursuit of pleasure and the flight from pain (see Troland, 1928, Chap. XVII). The ancient Epicureans and Stoics, as well as the English utilitarians (Bentham, J. S. Mill, and others), adhered to this doctrine in one or another form. Perhaps the climax of this theory's history was found in Bentham's "hedonistic calculus," an effort to reduce to quantitative form the individual's choice among lines of action involving pleasures of various amounts, durations, purities (i.e., freedom from admixture of pain), and other dimensions.

Volition.—Historically, the problems of voluntary or willed action have been almost hopelessly entangled with questions of ethics

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[2] An interesting neo-tropistic development has recently set experimental psychologists to comparing human beings with those self-correcting machines known as servo-mechanisms. See, for example, L. K. Frank *et al.* (1948). An example of an animal-like device of this sort is Hammond's "heliotropic machine" described in Loeb (1918, Chap. VI).

and metaphysics. The will has often been looked upon as a corrective to, or inhibitor of, the passions; and the question of its own determination or freedom has been a notorious source of bitter dispute and desperate heart-searching because of its implications for morality and religion. Such drama is strikingly lacking, however, in the treatment of the problem by the early experimental psychologists. Their tendency, as exemplified by Wundt, Michotte, and James, was to regard voluntary activity as distinguished introspectively by some sort of "action consciousness," "feeling of innervation," or the like. In dualistic or "double-aspect" theories of the mind-body relation, the voluntary act occurred when the idea of the act achieved the focal center of consciousness; this is "ideo-motor action" (James, 1890). For a structuralist like Titchener, problems of motivation would appear in this context; and the experimental problem would be that of ascertaining the factors that determine the entrance of such an idea into clear consciousness. In contemporary psychology, the distinction between voluntary and involuntary behavior is still sometimes drawn (Irwin, 1942), but experimental work is little affected by it.

The Nature of Motivation and Its Role in Theoretical Systems

In this section we shall characterize briefly the systematic positions of a number of psychologists with respect to the properties that they assign to motives and the functions of motives in their systems.

Guthrie and Holt. For Guthrie a motive has the properties of a persistent noxious *stimulus*. "It is not necessary to invent a desire for food," he says, "if we can show that the activity we are interested in orginates in hunger spasms in the stomach" (Guthrie, 1938, p. 92). Behavior similar to that produced by hunger spasms

> "could be produced by some artificial and external stimulus. A paper bag fastened to the cat's foot with a rubber band will similarly activate the cat, and it will become disturbed and excited and this state will continue until some one of its movements eventually removes the bag" (p. 96).

Holt differs from Guthrie primarily in regarding the *behavior,* rather than the stimulus, as the drive. So, speaking of hunger, thirst, sleep, defecation, etc., he says:

> ". . . Since their stimuli are contained within the organism, these cannot be avoided by any ordinary avoidance response (locomotion, etc.) but will keep the organism restless until it acquires, by trial and error, very different and often intricate modes of response which will allay the internal stimulation" (Holt, 1931, p. 125).

Positions that treat motives as persistent internal stimuli, or as behavior resulting therefrom, have the advantage of seeming very concrete, since everyone supposes that he knows what a stimulus is. They also handle easily the problem of the *initiation* of behavior, that being the common function of stimuli. However, as soon as human acquired or secondary motivation enters the picture, the simplicity and concreteness of these positions becomes difficult to reconcile with the subtlety, abstractness, and fluidity of the behavior. At present, the stimulus-centered theorists deal most successfully with the primary drives at the animal level. The reader should, however, acquaint himself with Miller and Dollard's (1941) attempt to extend the conception to human social behavior.

Hull. In some of his earlier writings (1930, 1937), Hull treated drives as stimuli. More recently (1943), however, he has adopted a different attitude. He starts with the functionalistic principle that there exist certain states of primary need, i.e., conditions under which food, water, a mate, escape from bodily injury, etc., are necessary for the survival of the individual or the species. These states of need are not in themselves psychological; but to account for the behavior that typically occurs in such a situation Hull postulates a psychological "intervening variable" of *drive.* This drive is related to antecedent conditions (e.g., number of hours of food deprivation) and to its consequences (e.g., running through a maze and obtaining food). Among the antecedent conditions there would be included some "drive stimulus" which would stimulate "drive receptors" whose afferent discharge would result in the arousal of the drive. In hunger, for example, gastric contractions

might be the drive stimuli, and proprioceptors in the stomach walls might be the drive receptors. The resulting drive of hunger, as an intervening variable, would have whatever properties it acquired from the functional equations relating it to antecedent and consequent "directly observable" variables, such as deprivation time and rate of running. One such property would be that of "strength."

In Hull's book of 1943, three principal functions are delegated to motives. (1) Learning occurs only when a response is *reinforced.* There are two forms of reinforcement, primary and secondary. Primary reinforcement of a response takes place when the response is made in temporal contiguity to the reduction of a drive. Stimuli that are present when primary reinforcement occurs become capable themselves of reinforcing responses; this constitutes secondary reinforcement. (2) "Reaction potential" (roughly, the tendency for a response to occur) is regarded as equal to the product of some function of "habit strength" and some function of strength of drive In Hull's notation this relationship is written:

$$_{S}E_{R} = f(_{S}H_{R}) \times f(D).$$

One consequence of the multiplicative relation between drive and habit strength is that a response will not be made if drive is zero, no matter how strong the habit may be. Note the resemblance of this principle to Lewin's contention that mere connections have no motivating power. (3) "Drive stimuli," like other stimuli, may have the properties of "cues." This means that the organism can learn to discriminate among its own drives, and may learn to make one response when one drive is present and another when a different drive is present. So, for example, Hull (1933) and Leeper (1935) showed that a rat could learn to turn one way for food in a *T*-maze when hungry and the other way for water when thirsty. Bloomberg and Webb (1949) and Jenkins and Hanratty (1949) found that rats could learn to respond differently under two different strengths of the same drive (hunger).

Tolman.—Since Tolman defines purpose as "a demand to get to or from a given type of goal-object" (1932, p. 452), his "purposive" behavior is *motivated* behavior. Purposes or demands he

defines objectively by two characteristics: (1) the *persistence* of behavior in getting to or away from goal-objects, and (2) the *docility* of such behavior, i.e., the tendency to select more and more efficient methods of getting to or away from goals. From this it might appear that purposes are properties of behavior. However, Tolman has also spoken of demands as "urges" (1932, p. 441), and has later given them the systematic status of intervening variables (1937, 1938). This implies that purposes are not properties of behavior, but rather are psychological processes resulting in, and inferred from, behavior—a view similar to Hull's.

Since Tolman has *defined* behavior as motivated, the concept of motive is a more fundamental one theoretically for him than for Hull, Holt, and Guthrie. In detail, however, Tolman uses particular drives, much as these other writers would, to account for the origination of behavior and its organization toward goal-objects.

Lewin.—The problems of motivation are treated by Lewin in his "vector" psychology (see especially Lewin, 1938).[8] Certain syndromes of behavior (e.g., that of a hungry person) represent states of need, whereas others (e.g., behavior of an intentional kind, such as reading a book), represent states of "quasi-need." When such behavior occurs, a region in a state of *tension* is said to exist within the individual. It is these tensions that constitute Lewin's concept of motives. He has been quite explicit about the properties of such

[8] In dealing with Lewin it is necessary to consider his relation to Gestalt psychology. He is commonly regarded as a Gestalt psychologist, and he so regarded himself. Like Wertheimer, Kohler, and Koffka, he was a "field theorist", and he occasionally employed the typical Gestalt concept of "good configuration." However, it is notable that he developed no physiological assumptions and made no use of the concept of isomorphism. Further, it is striking that the topological and "hodological" geometries that he employed for the representation of psychological situations are precisely geometries in which shape, form, or configuration in the sense of "Gestalt" has no significance. The difficulties attending the attempt to incorporate Lewin's concepts into the general framework of Gestalt psychology are very evident in Koffka's *Principles of Gestalt Psychology* (1935). Comparing the systems of Lewin and Koffka, MacColl (1939) concludes that they are too different to permit a mixture of their concepts. In particular, she points out that their uses of the concept of "tension" are mutually incompatible and she doubts that there is any place in Lewin's scheme for the distinction between stable and unstable psychical systems, in spite of Zeigarnik's (1927) employment of these notions. We conclude that Lewin, although a field theorist, cannot properly be classified among the orthodox Gestalt psychologists.

tensions: (1) tension in any region tends to change in the direction of equality with that in adjacent regions; and (2) a tension implies forces at the boundaries of the region in which the tension exists (1938, p. 98).

Tensions are "dynamical constructs" (psychological intervening variables), and are employed to account for the relations among the observable facts of behavior. The concept of tensions has for Lewin no physiological connotations nor anatomical localization, although he does not object to attempts to use physiological tensions as symptoms of his psychological ones. Tensions are coordinated in his theory with those patterns of behavior that are generally recognized as representing drives or motives; and the discharge of a system under tension is coordinated with the satisfaction of a drive or motive.

An important aspect of Lewin's tensions is that they are not directional, and that they are only indirectly related to what he calls the "motoric"—i.e., the processes that govern the machinery of bodily locomotion. The peculiarly loose relationship between tension and locomotion that is described by Lewin is highly characteristic of his system. An example or two will assist us to understand this relationship.

> Let us suppose that a child who likes candy is in a room, and that he sees an open box of candy. The desire for candy means that a region under tension exists within the child. When the child perceives the box of candy, he can be expected to move toward it, if his powers of locomotion suffice and if he is not restrained by impassible barriers or repelled by opposing forces such as fear of punishment. This locomotion toward the candy would be expected no matter where the child is in the room or where the candy has been placed. The candy would then be said by Lewin to have a *positive valence* for the child. Any object that has a positive valence attracts the person from all points in the field, and is thus the center of a "force field" in which forces are directed toward the object from all points in the field.
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> If, on the other hand, the child finds himself in the presence of a small dog of which he is afraid, he can be expected to move away from the dog, regardless of the actual positions of himself and the dog, if he is able to do so and if barriers or opposing forces do not prevent it. The dog, then, has a *negative valence*

for the child, and is the center of a force field in which forces directed away from it exist everywhere in the field. In this example, the region under tension is that which represents the child's fear of the dog.

In general, then, any object or situation whatsoever has a positive valence if it is the center of a field of attracting forces, and a negative valence if it is the center of a field of repelling forces The strength of the forces in the field surrounding a valence will vary, other things being equal, with the strength of the valence. In addition, however, these forces may vary with other factors, such as distance; thus, Lewin supposes that generally the forces decrease as distance from the valence object increases. The candy may become more attractive for the child as he approaches it, and the dog less fearsome as the child moves away from it.

The existence of a valence always corresponds to the existence of a region under tension in the person, and variations in this tension will produce corresponding variations in the strength of the valence However, it is possible for a tension to exist without any corresponding valences. Such a tension might manifest itself simply in "aimless" or restless activity, as when one "wants something but doesn't know what he wants," or when a rat exhibits apparently undirected activity as the first sign of incipient hunger. Further, valences depend upon the cognitive structure of the situation. A mere box may have no valence for a child, but it quickly acquires one if he is told that it contains candy.

Locomotion itself is determined by the resultant of whatever forces are acting upon the person at the moment. Lewin does not, however, believe that the physical "parallelogram of forces" applies generally to the determination of the resultant of psychological forces. It remains to be determined experimentally what are the laws of such psychological resultants.

Freud.—Psychoanalytic doctrine has made more numerous and important contributions to the theory of personality structure and "dynamics" than to motivational theory. However, Freud exerted two extremely significant influences upon the latter field First, the whole weight of his system, which has become, whatever its merits.

an integral part of Western culture, was thrown behind the view that motives are to be looked for in all behavior, even the most apparently purposeless or accidental Second, he asserted vigorously that these motives are known to their possessors only in small part, and that behavior will be understood in only the most superficial way unless its unconscious determiners are discovered Since Freud insisted upon a deterministic psychology, even though motivation was a cardinal concept in his system, and since his stress upon the inadequacy of an individual's understanding of his own motivation amounted to a devaluation of introspective explanations of behavior, psychoanalysis is more sympathetically related to the general position of the deterministic behaviorists than might at first be expected

Freud's views on the nature of motivation are found in their explicit form in his discussions of the "instincts" (German *Triebe* perhaps better translated as "drives" or "impulses") He himself was evidently never satisfied with his theory of the instincts, which he regarded as "the most significant, but the most incomplete part of the psychoanalytic theory" (1938 p 576 note 1) His treatment of the problem varied greatly with the level of speculation at which he wrote

(a) When he was holding the reins loosely (Freud 1927), he thought of the instincts as tendencies to return to a previous state —tendencies characteristic not only of the living organism as a whole, but of the very particles of which the living being is composed Eros—the sexual and self-preservative instinct— tends to restore equilibria of the organic state that have been disturbed, while the death instinct leads organic matter back toward its non-living state

(b) At a less imaginative level he wrote of the instincts as forces or energies. Here he often used hydraulic analogies, including the figures of speech of "reservoirs" and "communicating pipes"; on other occasions, however, the instincts are personified, and are said to "seek expression" as one might say that water "seeks its level." The instincts are, however, energies that are *directed* toward some object.

(c) Finally, when he was most in the mood of the natural scientist, Freud wrote (1933, p. 132), much in the vein of Holt's and Guthrie's treatment, that the instincts are the psychological result of continuous inner somatic stimulation that, because it *is* internal, cannot easily be gotten rid of. The instincts themselves are not qualitatively different, he said; but they are distinguished by having different somatic sources and different aims or objects.

Of these three levels of theory, the second is perhaps the most pervasive in Freud's writing, and the most characteristic of his theory in its more general contexts. Whether or not the "directed energy" notion can be thought of as always being reducible to the more experimentally manageable "somatic stimulus" notion, there is nothing more Freudian in quality than the devious, amebic and subterranean wanderings of the "libido" in search of an outlet. At the same time, the nature of the libido and its relation to sexuality are left open and uncertain with even the closest attention to Freud's words. He himself has called the theory of instincts the "mythology" of psychoanalysis (1933, p. 131), and one can gain the impression of greater understanding by withdrawal from the details of the text, as one might search for the meaning of poetry within one's feelings rather than in a textual analysis. It is doubtful, indeed, that Freud developed a fully consistent theory of the libido, even if one studies his views at a single period of his writing.

In his *Three Contributions to the Theory of Sex*, Freud defined libido as having the same relation to the sex instinct that hunger has to the instinct for taking nourishment. Later, he tended to extend the use of the term "sexual" as he was extending the scope of the concept of the libido, and eventually the notion of sexuality was applied to all processes whereby pleasure was obtained from various parts of the body, among which the genitals constituted but one of the erogenous zones. Further extension was then made to pleasure gained from objects outside the body, from other persons, and even from ideas. The libido emanates, then, from various bodily sources, whose energies are capable of fusing into the maturely sexual form; Freud speaks of this as the combining or fusing of instincts. Whether the self-preservative, as well as the race-pre-

servative, instincts contribute to the libido was still not clearly decided in Freud's last writings (Freud, 1949), although we see there at its clearest his reduction of all the instincts to the fundamental two, Eros and the death instinct.

The role of motivation in Freud's doctrine is a central one; one might even maintain that psychoanalysis is devoted entirely to demonstrating the subtly indirect means whereby motives develop the structure of personality and use it to achieve their satisfaction. In his earlier views, Freud assumed a hedonistic position: behavior is directed toward maximizing pleasure and minimizing pain, in accordance with the "pleasure principle." Pleasure represents the abolishing or reduction of excitation, and pain, the arousal or increasing of excitation, in the nervous system. Since the instincts provide the most constant source of excitation, and the most difficult source to eliminate, behavior is largely concerned with reaching the objects of the instincts, i.e., those goals in or through which the instincts can achieve satisfaction, so that the corresponding excitation is eliminated. The fact that behavior is variable from person to person results from the fact that the objects are not originally attached to the instincts, but become so because they provide means of satisfaction; and such satisfaction can be provided in a variety of ways. The attachment of libidinal energy, or perhaps *any* instinctive energy, to a given object is called *cathexis* (Greek, a holding or occupying). A major aspect of Freud's theory of personality development deals with the temporal sequence of objects commonly cathected by individuals from infancy onward.

Later, as Freud's notion of the structure of personality became more complex, he was led to modify this simple hedonism. On the one hand, he convinced himself that an instinct of destruction, the death instinct, existed as a fundamental force aiming toward the return of organic matter to its inorganic state. Also, like other hedonists, he had to cope with the fact that much of life is not pleasant, and that much behavior actually increases the quantity of the present pain. To deal with these facts, he invented the *reality principle,* which operates somewhat in the manner of Bentham's "hedonic calculus." The reality principle permits one to take into account both the present facts and the future possibilities of a situation, so

that lesser pleasures of the moment may be sacrificed for greater ones in the future. The *id* is governed by the pleasure principle, and the *ego* by the reality principle. Freud never satisfied himself, however, as to whether genuine violations of the pleasure principle were possible (cf. Freud, 1949).

Origins of Motives

Motives are commonly distinguished as *primary* and *secondary,* the former being those that appear to be dependent upon innate or constitutional conditions, and the latter, those that are acquired through experience and learning

Conditions of the Primary Motives. Most discussions of the primary motives, like the one of Hull as described above, begin with elementary biological facts If organisms and their species are to survive, it is clearly necessary for them to obtain supplies of materials for energy and growth; to avoid bodily damage from injurious objects, other organisms, and extreme environmental changes; and to meet with members of the opposite sex In this non psychological sense, organisms cannot avoid falling into states of biological need, i e , absence of conditions necessary for survival or presence of conditions detrimental to survival

The primary motives in the higher organisms are somehow related to these various biological states of need; and it is a problem for the psychologist to discover the nature of these relationships It is important to note at the outset that no perfect correlation exists here. A biological need may exist without the coming into being of its appropriate primary drive; and, likewise, the presence of a primary drive does not guarantee the existence of the corresponding bodily need. To take but one example, one's health may well be jeopardized by a loss of appetite, when the body needs food, or by a craving for food beyond the demands of the body. Much confusion in the theory of primary motivation has resulted from failure to remember this fact The situation has been aggravated by the unfortunate, but now unavoidable, terminology that applies the term "need" to a biological condition on one occasion, and to a motive on another occasion, sometimes within a single paragraph.

Keeping this distinction in mind, we turn to theories that attempt to show how the primary motives are related to need-states.

Homeostasis and Least Action. As a first step toward the solution of this problem, Cannon (1932) has described a variety of physiological adjustment-patterns whose function is to maintain certain conditions within the body at a nearly constant level. These adjustments he has called "homeostatic processes." A human being's life depends upon the efficiency of these processes and is immediately endangered if his body temperature departs more than a few degrees from 98.6° F, if the hydrogen-ion concentration (pH) of his blood varies much from the normal of about 7.4, if his blood sugar is too high (hyperglycemia) or too low (hypoglycemia), if the oxygen supply to his blood is deficient (anoxemia), or if a disturbance occurs in any of a large number of other equilibria within what Claude Bernard called the "internal environment." Each of these equilibria tends to be maintained by automatic action in which the endocrine glands and the autonomic nervous system play prominent roles. Such action is limited in its scope, however, and the individual may have to seek a warmer or cooler place, a source of energy to replace expended materials, and so on. This searching will appear as motivated behavior.

Freeman (1948) has asserted that all motivated behavior may be regarded as homeostatic. This is a relatively specific form of a view that has been expressed in many ways. Tolman, for example, asserts that the ultimate goal of behavior is the elimination of some disturbing physiological condition or the avoidance of a prospective one. Lewin's tension systems, by definition, tend toward equilibrium with their neighbors. Within and outside of psychology, quiescence, equilibrium,[4] and Nirvana have been seen as the final ends of all endeavor. However, this conception shares certain difficulties with other highly general doctrines of motivation, such as hedonism, the principle of least action (e.g., Wheeler, 1929), and Freud's Eros and death instincts. When an independent criterion of homeostasis, pleasure and pain, least action, or the Freudian instincts, is not

[4] Köhler (1938, Chap. VII) has argued that the term *equilibrium* is incorrect in this context, and that it is *stationary states* that are maintained in an organism.

available, these doctrines tend to be no more than articles of faith; they are prone to take whatever the organism does as the criterion of what is homeostatic for it, pleasurable for it, etc.[5] The question must be raised whether these are scientific hypotheses at all, in the sense that they are in principle capable of disproof by experimental evidence. If, like Tolman, one supposes that behavior always serves some hidden physiological economy, the principle becomes barren, as he himself says, since inability to determine this physiological economy robs the principle of explanatory power.

Instincts Versus Drives.—A long-standing controversy has centered about the extent to which organisms possess innate capacities that, without learning, enable them to meet their needs. James and McDougall are but two of the influential older psychologists who attributed to animals as many instincts as seemed to them necessary to account for the common types of behavior that characterize various species. Instinct theory was often peripheralistic; the presence of a certain kind of stimulus, such as a female of the same species, was believed to produce, by innate determination, a certain kind of response, such as pursuit, courting, and mating. Inborn sensori-motor connections, or sometimes chains of reflexes, were frequently hypothesized as the physiological basis of instinctive action. The behavioristic revolt, however, generated a skeptical attitude toward all forms of innate determination. An extremist like Kuo (1924), for example, denied altogether the existence, not only of instincts, but of any inherited forms of behavior; and Holt (1931), likewise, contended that even the reflexes were learned, either before or after birth. In addition, the influences of Gestalt theory and of such studies of the nervous system as Lashley's (1929) were set squarely against connectionism, innate or learned.

The outcome of this controversy has been the substitution of the concept of *drive* for that of instinct, especially in the human being and the higher animals. This represents (a) a greatly diminished reliance upon hypothetical innate sensori-motor connections

[5] Before accusing these doctrines of circularity the reader should acquaint himself with Meehl's (1950) defense of the law of effect against this charge. Zipf's (1949) use of the principle of "least effort" as a hypothesis from which testable deductions can be drawn is also relevant here.

as the explanation of sex behavior, nest-building, migration of birds and insects, and other behavior that is typical of various species, and at the same time (b) an emphasis upon the adaptive variability of such behavior. The observed adaptability of supposedly instinctive behavior and its failure to conform to rigidly stereotyped patterns have been difficult to understand in terms of innate connections. The concept of drive, on the other hand, seems better able to deal with behavior that fulfills common and, often, essential, functions, but does so in ways that vary according to the circumstances. Instincts imply that the organism is innately prepared to do the right thing when a need-state arises; drives imply that the organism is impelled to learn what to do.

Some years ago, Pillsbury (1927) pointed out that the innateness of instincts might come into play at any one of three points: in the instigation of movement, or in the prepotency of some movements over others in learning, or in the "test of acceptability" that governs the selection of goal objects. To these we might wish to add a fourth point, a possible innate selective power in perception like that suggested by Lorenz in observations on animals' responses to "releaser" stimuli (Tinbergen, 1948). Much futile debate over the instincts might have been avoided if it had always been clear just what aspect or determiner of behavior was assumed to be innate. While many psychologists today are still reluctant to accept innateness anywhere, without overwhelming evidence, distinctions like Pillsbury's are gaining more attention. The work of Beach (1942, etc.) clearly exemplifies this. Furthermore, the pendulum of opinion has definitely swung away from the extreme position of the Watsonian behaviorists toward an acceptance of nonlearned determiners of behavior, as can be seen in the papers of the *Symposium on Heredity and Environment* (Beach *et al.*, 1947).

Specific Theories of the Primary Drives.—*Hunger and Thirst.*—Cannon's so-called "local" theories of hunger and thirst have so overshadowed this field that their validity is often taken for granted. They are based, to begin with, upon the observed parallelism between reported experiences of hunger and thirst, on the one hand, and certain bodily states which he believed produced the stimuli for

these experiences, on the other. Cannon and Washburn (1912) and others have offered convincing evidence that reportable hunger pangs are closely correlated with the presence of stomach contractions. Likewise, Cannon related the experience of thirst to dryness of the mouth and throat.

Similar to these accounts of hunger and thirst are the peripheralistic theories that regard sexual and eliminative drives as conscious urges due to stimulation of the hollow viscera, such as the seminal vesicles, bladder, and colon, by distention.

There exists at present a trend away from such theories, and toward searching for a basis of primary motivation in hormones or other chemicals in the blood stream. Morgan's (1943) discussion of the physiology of motivation is a good illustration of the tendency to put less stress upon peripheral mechanisms in the gastric tract, esophagus, and sexual apparatus, and to substitute for them hypothetical sensitizing agents directly bathing the central nervous system by way of the blood.

Evidence that seems clearly to restrict the scope of the local theory of hunger may be mentioned briefly. Bash (1939) found that the hunger drive persisted in his rats after he had operated upon them so as to remove the afferent neural connections of the stomach. Further, Tsang (1938) removed most or all of the contractile portion of rats' stomachs without eliminating the hunger drive. In both experiments, some disturbance of hunger behavior appeared; but a good deal of explaining away has to be done if the local theory is to be saved.

As for the local theory of thirst, the evidence from Montgomery's experiments (1931), in which his dogs drank normal quantities of water after removal of the salivary glands (which, by the local theory, should have produced dryness and excessive drinking), is clearly unfavorable. So also are the data from sham-drinking experiments, in which dogs drank amounts of water proportional to their needs as previously measured, when the water never reached the stomach, being led out through a fistula from the esophagus.

The fact that animals and humans stop eating and drinking long before the water and food can have had opportunity to supply bodily needs by being assimilated into the tissues is as obvious as it is

often disregarded. To what extent this stopping is learned during the lifetime of the organism is a problem as yet unsolved; but a theory of satisfaction is just as necessary as a theory of instigation.

Sex.—The sex drive differs from hunger and thirst in its biological role, since it concerns the preservation of the species rather than the individual. There appears to be no "tissue need" behind the sex drive, so that the problems that it sets for psychophysiology resemble those of so-called "sensory" and "activity" drives, rather than those of hunger and thirst. Theorists have stressed various aspects of sexuality, depending upon their interests and the scope of their observations. Some have been impressed by the clear patterns of behavior in pursuit, courtship, sex play, and copulation, that are exhibited by many lower species without opportunity for learning. Others have claimed an important role for peripheral factors such as pressure of fluid in the seminal vesicles or the sensitivity of the genitalia. Still others have looked to hormones of the gonads and other endocrine glands for the answers to these problems. Finally, there are many who have felt that, at least in the human being, all of these factors are insignificant when compared with the influences of experience and learning. It would be foolhardy to attempt to evaluate theories of sexuality in anything less than an extensive monograph. The student who wishes to read the literature should, however, be forewarned of two ubiquitous sources of confusion: first, unjustified generalizations from animals to human beings; and second, failure to observe the distinction between reflex and other behavioral mechanisms, on the one hand, and the sex drive that they serve, on the other. Thus, it is scientifically interesting to demonstrate that an inexperienced male rat will pursue and copulate efficiently with a female in heat, but this by itself tells us little about what is necessary to arouse, maintain, and abolish this behavior of the male rat, and tells us nothing at all about the nature of the motivation of the human male.

Elimination Needs.—Whether micturition and defecation constitute basic motives seems doubtful. It seems more likely that whatever motivation occurs in this connection is the result of training that permits these acts only under certain circumstances, so that

the individual has an avoidance motive against performing them in inappropriate situations and must therefore seek suitable occasions.

Sensory and Activity Drives. Murphy (1947) has objected to the restriction of the primary drives to those connected with the viscera, and has argued for the existence of sensory and activity drives. These would include drives toward the obtaining of sensory and perceptual experiences, and toward muscular activity, rest, and sleep. It is of interest in this connection to note that Harlow, Harlow, and Meyer (1950) believe that a "manipulation" drive is demonstrable in the rhesus monkey.

Avoidance Drives. The negative or avoidance motives are seldom given special theoretical treatment. Tolman has pointed out that, while the appetites tend to be cyclical, as shown by Richter (1941) and others, the aversions stand ready to become active immediately upon the presence of a threatening stimulus. Hull refers to the fact that the "need to avoid tissue injury" is likely to be satisfied by reflex activity, whereas the hunger, thirst, and sex needs commonly require preliminary search before consummatory action is possible. It should be added that an important concept for Hull is that of "reactive inhibition." This is a primary avoidance motive which is supposed by Hull to be created as a result of any action of an organism; it constitutes a drive away from performance of the action that creates it. (Compare the discussion of "satiation" below in the section on Substitution.)

Specific Appetites.—It has been observed that organisms deprived of adequate supplies of particular dietary elements, such as sugar, salt, phosphorus, calcium, certain vitamins, etc., may develop cravings for these elements (see Morgan, 1943, pp. 451-455). Further, in a number of experiments, animals (including human infants) that were permitted to select their own diets from an array of foods have been found to make selections adequate to their nutritional needs; indeed, if this were not so, it is difficult to see how animals could survive "in a state of nature." The existence of such specific appetites raises the question whether it is proper to conceive of a general hunger drive, rather than a multiplicity of related but specific drives.

The theory of the specific appetites is not far advanced. Reliance upon learning theory leads some writers to suppose that animals in a state of deficiency select needed elements through trial-and-error. Against this supposition, in some cases, is the apparent lack of opportunity for such learning. It is also frequently difficult to understand how ingestion of needed materials can be followed by assimilation and relief quickly enough to provide a basis for discrimination. Another theory suggests that a state of bodily need lowers the sensory thresholds for particular olfactory or gustatory qualities. However, Pfaffman and Bare (1950) found no such change; and one might also ask why a lowered threshold would not lead to *less*, rather than *greater*, consumption of the material.

Origin of Secondary Motives. Since most human behavior is supposed to be motivated by secondary drives, largely social in character, it becomes an exceedingly important theoretical and practical question to determine how the secondary motives originate.

Means Become Ends.—One suggestion can be summarized by the statement that "means become ends." Thus, Williams' (1929) rats learned a maze in which presumably their only reward for reaching the end was entrance into a goal-box in which they had been given food in an earlier discrimination-learning procedure. However, in later trials the rats ran to the empty box less rapidly and directly. Similarly, Wolfe's (1936) chimpanzees did considerable amounts of work for poker-chip tokens that were exchangeable for food, play, and other goals, and discriminated among tokens of various values and of no value. At least one ape "begged" for such tokens, successfully, from another ape in a peculiarly human fashion. Wolfe did not report data on "extinction" of these responses by cessation of the accustomed rewards.

Canalization.—Closely related to this notion is the concept of "canalization," which Murphy has dealt with very extensively as a key principle of the development of personality. His condensed statement of this principle is that "needs tend to become more specific in consequence of being satisfied in specific ways" (1947, p. 161). An example of Murphy's use of this concept is his description of the canalization of an infant's needs upon its mother, in view of

her status as an early satisfier of a multitude of needs, both visceral and sensory. Only a study of his own discussion, however, can do justice to the penetration and flexibility, as well as the generality, with which he has employed the notion.

Murphy has distinguished the process of canalization from that of *conditioning* on two grounds. First, in conditioning, the conditioned stimulus is not a satisfier: while a dog might go so far as to lick a light bulb to which it had been conditioned under the hunger drive, the bulb or light does not satisfy the hunger. Second, he believes that canalizations, unlike conditioning, are not subject to extinction. New canalizations continually appear during the life history of the individual but the old ones remain. If a liking for jazz is supplanted by a love of Bach, the earlier taste continues to exist, although its expression may be inhibited by "secondary habits," such as a fear of disapproval.

The theory of canalization rests upon a small amount of experimental evidence and a large amount of plausible illustration from general experience. For an adequate experimental test of the theory, we must decide whether its central idea is that needs become more specific or that goal objects become more and more adequate as satisfiers. If the former is the case, we need evidence not only that the drive toward a given class of goal objects becomes stronger, but that the ability of other goal objects to act as satisfiers becomes lessened at the same time. Even if the individual tends more and more to select a particular class of goal objects, we must distinguish between the case in which the person makes this choice because he has learned that these objects are satisfying and obtainable by well-known means, and the case in which these objects now give more satisfaction than they formerly gave. Can we then agree with Murphy (p. 162) that "To investigate canalization experimentally is not difficult"?

Secondary Reinforcement.—Hull's principle of secondary reinforcement states that stimulus-response connections are strengthened not only by occurring at the time of need reduction (satisfaction), but by occurring together with any stimulus that has been closely associated with need reduction. From this point of view, the fact that Williams' (1929) rats learned a maze when they found at its

end only an empty box was due to the secondary reinforcing power the box had gained in its earlier association with food, while the eventual loss of efficiency in maze-running represented extinction resulting from the continued nonreinforcement by food This principle is an important one for Hull's systematic position. However, *although it may be used to account for an animal's seeking or* avoiding a formerly neutral object, it does not imply the development of a new *motive*. The behavior is carried out under the original motivation and disappears when it consistently fails to result in satisfaction of that motivation.

The Miller-Dollard Hypothesis Miller and Dollard (1941) and Miller (1948a, 1948b) have dealt in a detailed way with the "acquiring of drives." For them, a new drive is acquired or "learned" by the subject's making certain responses that produce stimuli that by association with original native stimuli, bring about a certain drive. Thus, to use an example of theirs, a child in the dentist's chair is strongly motivated to get out of the chair Whenever the pain of the dentist's drilling stops or is reduced, all responses occurring at that moment are rewarded so that they are more likely to occur the next time the child is hurt. This includes whatever anxiety responses have been going on By generalization, the various nonpainful aspects of the situation become anxiety-producing, so that the very mention of the dentist's name may bring them about, and together with them the tendency for whatever avoidance responses have been rewarded in such situations in the past The child now has acquired a drive to avoid the dentist and his office Miller and Dollard point out, however, that the mechanism of the drive itself is innate; what has been acquired is the tendency for a previously neutral situation to elicit the drive The only difference between primary and acquired (or "acquirable") drives is that: "Acquirable drives can be connected to cues according to the principles of learning; primary drives cannot" (Miller and Dollard, 1941, p. 57, n). If learning can cause a new stimulus to elicit the drive of anxiety, then anxiety is acquirable; if this is not true of hunger, then hunger is a primary drive In the case of acquired drives, then, we might better say that what is acquired is a connection between stimuli and drives, rather than drives themselves.

It is of interest here to note that E. E. Anderson's (1941) principle of *externalization of drive* asserts that drives may themselves become elicited by newly associated stimuli. If this is true of all drives, then no drives are primary in the sense of Miller and Dollard. The resemblance between Anderson's principle and Katz's (1937) "two-component theory" should be noted.

Differentiation. –Lewin's treatment of motivation was initiated by the study of human subjects acting under motivation induced by instructions or tasks which they had been given, and to a large extent his individual psychology (leaving out of account his social psychology and his concept of "power fields" induced by one person around another) dealt with behavior of an intentional nature. He distinguished such "quasi-needs" from primary or biologically determined needs, but has left little in the way of discussion of the development of the quasi-needs. The needs and quasi-needs are said to correspond to regions within the "psychological person," a region that has its own structure and that becomes more highly differentiated as the individual matures. The early work of his students (Zeigarnik, 1927; Ovsiankina, 1928; Karsten, 1927; and others) can be regarded as a demonstration that the quasi-needs have the same properties as do the primary needs (persistence, satiation, tendency to spread through the person, etc.) Changes in the structure of the person, which will mean changes in the needs and quasi-needs, arise, Lewin (1946) says, through both differentiation and fusion; and he accepts the possibility of autonomy in Allport's sense.

Functional Autonomy –In his *Dynamic Psychology*, Woodworth (1918) contended that the secondary motives arise out of behavior that originally leads toward the satisfaction of the primary motives. Specifically, various (neural) elements which once were involved in the process of bringing about behavior under primary motivation themselves acquire motivating power: "Mechanisms become drives." G. W. Allport (1937), impressed by the strength and variety of the motives that characterize different individuals, has developed a somewhat similar position and has made his notion of the "functional autonomy of motives" a cornerstone in his conceptual scheme for the understanding of personality. The essential feature of this concept is that, while new motives may and, no

doubt, do grow out of old ones, the new become functionally independent of the old, to the extent that only a historical connection may finally exist between them. Allport has sought out a variety of evidence to bear upon this problem, from the "circular reflex" of Holt to the phobias of abnormal psychology. On the whole, the argument tends to reduce itself to the proposition that means become not only ends but ends-in-themselves. How otherwise can we account for the motivation of the hero, the martyr, the miser, the man who is truly devoted, whether to chess, to Brahms, to his fellow-man, or to God? Only by such a theory, Allport believes, can we avoid the superficiality of stimulus-centered theories that overlook the urge-like quality of motivation, or the absurdity of reducing the motives of the mature adult to the organic drives, so that one might have to contend that stomach contractions or pressures in the hollow gut are genuinely responsible for the *present* drives of the composer toward his symphony, the mathematician toward his new development in number theory, or the collector toward his acquisition of a rare stamp or match box. This emphasis upon consideration of the "contemporary" nature of motivation is obviously sympathetic with Lewin's insistence upon the nonexplanatory character of historical data. At the same time, it opposes the conditioning and learning theories of secondary motivation and such historically-oriented attacks as Freud's excavation of an individual's dim past for the explanation of his present neurosis.

Vigorous objections to this concept have not been wanting. The factual evidence in its favor is far from conclusive, and the apparent absurdity of referring complex values to the functioning of the viscera is at present an article of faith rather than a well documented conclusion. Murphy (1947) has objected that the principle of autonomy does not enable one to determine what will become autonomous and what will not. McClelland (1942) has set forth some of the conditions that evidence for autonomy must meet, by describing a number of situations in which the concept is likely to be applied quite erroneously. For example, the behavior may now be based upon a motive different from the one that originally brought it about; the absence of reinforcement under the original motive may be apparent rather than real, because of a failure to analyze

the present situation adequately; the behavior may as a matter of fact now be in process of extinction, but not yet extinguished; or the original motivation of the behavior may still be present, having failed originally to be identified correctly. To avoid these sources of erroneous conclusions is clearly a difficult feat

Strength of Motives

Motive Strength as a Concept. Only the most formal systematizers have undertaken to deal explicitly with the definition of motive strength, most writers have taken the concept for granted. Hull (1943) has employed drive strength as an intervening variable, and has stated that, like other intervening variables, it must be defined by two sets of equations, one set giving motive strength as a function of antecedent conditions such as deprivation time, and another set giving such consequents as "vigor of organismic action" (p. 66) as functions of drive strength In addition he points to the need for objectively defined units of measurement of this variable

As may be recalled from our earlier discussion, Lewin sees the relation between motive ("tension") and action as more complex than this. For him, tensions differ in degree, but this difference is reflected in the vigor of behavior toward a goal only by way of changes in the valences that correspond to the tension Thus, what one usually does is to measure *forces* (i e., valences); but this represents a measurement of tension only if it can be presumed that these forces are varying with nothing but the changes in tension Referring again to the definition of tension given in the section on The Nature of Motivation and Its Role in Theoretical Systems, it appears that a direct measure of tension would require us to measure either the strength of forces at the boundaries of systems under tension, or the rate or extent of spread of tension from one region to another. If this is accepted, then such symptoms as the effects of a motive upon activities not immediately related to achievement of a goal (e g., stuttering in anger at an interfering person) are more fundamental criteria of the strength of a motive than are symptoms of the vigor of behavior that is directed straight toward the goal (e.g., rate of locomotion).

Criteria of Strength and Methods of Measurement.—The theoretical problems of methods and criteria available for measuring strength of motives are largely technical. We shall discuss briefly a few of them, borrowing freely from Lewin's work on the measurement of psychological forces (1938).

Rate of Locomotion.—An experiment by Bruce (1937) provided data on the rate at which thirsty rats ran a 17-foot runway to a water container. The animals had been deprived of water for about 24 hours. Just before being placed in the runway, they were given a certain percentage of their normal daily water ration. It was found that the smaller the percentage of their ration they were given, the faster they ran. The relationship was quite regular except for the fact that rats that were given a small amount of water (5 per cent of their ration) ran faster than those that were given no water. Although at first Bruce's experiment seems quite simple psychologically, a number of questions can be raised.

1. If the rat is thirsty at all, why does he not always run as fast as he can? In other words, do we not have to postulate some sort of inertia that must be overcome by different drive strengths, if we are to understand the graded, rather than all-or-none, nature of the relation between drive strength and rate of locomotion?

Hull (1938) contends that the work done in responding builds up a need for rest from responding. This need constitutes a drive that opposes further work, and is called reactive inhibition. It would follow that the rats' rate of locomotion depends upon an equilibrium between this reactive inhibition and the strength of their thirst drive. That is to say, the rats will not fatigue themselves any more than their thirst demands.

Lewin has handled the problem somewhat differently. He distinguishes between "driving forces" (positive and negative valences) and "restraining forces." The latter are *resistances*, analogous to the physical resistance that soft sand offers to a person walking through it. The sand slows down the person's progress, but does not actively drive him away from the sandy region. Lewin would hold that the locomotion of the rats is acted against by such a restraining force, rather than by the driving force that Hull assumes.

This distinction will perhaps become clearer in our discussion of the obstruction method.

2. We have mentioned that the rats ran somewhat faster when they had been given a small percentage of their water ration than when they had received none at all. This effect of a "foretaste" resembles the case of a person whose appetite is whetted by the smell of food, or whose craving for tobacco or music is aroused by seeing someone smoking or hearing the name of a composer. Such concepts as "attention," Lewin's (1938) "potency," Anderson's (1941) "externalization of drive," or perhaps Hull's (1949) "stimulus intensity dynamism," may be relevant to these observations.

3. Aside from problems of motive strength Bruce's results indicate how quickly the act of drinking affects water-motivated behavior. The rats' tissue-needs for water can scarcely have been supplied in the short time that was adequate for effects upon behavior to be very pronounced. Hull (1943, pp. 98-99) has suggested that such consummatory responses as eating and drinking become powerful reinforcing agencies by the principles of secondary reinforcement. However, in the present case we have to deal with a *reduction*, rather than an increase, in the vigor of the behavior.

4. We shall pass over the fact that the rats ran the alley, even when given their whole ration This raises technical problems that would require extensive discussion

5. For the purposes of studying human motivation, rate of locomotion is often too greatly affected by planfulness and social factors. A mile runner would not expect to make his best time for the whole mile by running the first hundred yards like a sprinter; and a dignified man might prefer missing his train to being seen running for it.

Overcoming Obstructions.—In 1924 Moss reported that an increasing percentage of a group of rats would cross an electrified grid to food as the length of their fasting-time was increased. Attempts to determine a threshold strength of shock—an amount that would be just strong enough to prevent rats with a certain deprivation period from crossing the grid—have been unsuccessful because of the great variability of the behavior in the shock situation

However, an electric grid obstruction box was used with considerable success in a long series of intensive investigations of drive strength by a group at Columbia University under Warden (1931). In these studies the shock was kept at a constant, relatively low, level, and measurements were made in terms of the number of times the animals approached and crossed the grid during a 20 minute period. It is clear that this method presumes: (1) that the animals know, by perception or from previous training, that the goal-object is in the incentive chamber on the far side of the grid, (2) that the animals must not be able to get beyond the grid except by a relatively uniform action that always results in shock, and (3) that the time allowed does not permit any considerable changes in the motivation of the animals, by satisfaction, fatigue, adaptation, etc. These requirements were reasonably well met by the method as used.

The major results of these experiments are well known. The number of crossings of the grid to food, water, and an animal of the opposite sex increased with deprivation up to a period of two to four days, after which it decreased. Furthermore, the maximum number of crossings made under each drive fell in the following order, from greatest to least: maternal (in the female), thirst, hunger, sex (in male and female), and exploratory.

It is easy to think of this method as analogous to measuring a physical force by matching it against known resistances or opposing forces. Lewin (1938) has shown, however, that this resemblance is quite superficial. Let us suppose that, as is true, the time required for passage over the grid is short, that the time consumed by the animal in the incentive chamber and by the experimenter in returning him to the starting chamber is also short, and that both are nearly constant throughout the 20 minute period. What, then, is the time that varies so much from one condition to another? Clearly, it is the interval extending from the moment the animal is replaced into the starting chamber until the moment he starts across the grid. And why does the animal not cross immediately, if he is to do so at all? One seems forced to assume that something within the animal is changing during this interval, and that he will once more attempt the obstacle only when a sufficient change has occurred. But what is sufficient for a strongly driven animal is not so for

another less motivated, so that the latter will, in the end, make fewer crossings In other words, we are measuring what, in the human being, would be called "decision time" Lewin suggests that the changing "internal" conditions may be "chance" variations of psychological state, the acuteness of the memory of the shock received at the grid or the frustration of failing to be satisfied after crossing it, energy or fatigue conditions, or some sort of adaptation No doubt other possibilities exist; we do not know, at present, what the answer or answers will be Nevertheless, the reduction of the method to a latency or decision-time criterion seems inescapable

The electric-grid obstruction method has been criticized because of the disturbing effect of shock upon the animals In Lewin's terms, it is likely to act as a negative driving force, rather than a restraining force It has not been shown that this introduces a biasing effect but it must contribute to the variability of the results Obstructions consisting of sand to be burrowed through, or sheets of paper to be gnawed through have occasionally been used. In these cases it appears that the obstacle corresponds to a "restraining" force, rather than to the negative "driving" force set up by the electric grid

Choice between Goal Objects If a subject is required to choose between goal objects that pertain to different motives, the choice will presumably be determined in part by the strengths of these motives However, without control over the preferences for the particular goal objects that are used, no conclusion about the relative strength of the two motives can be drawn Thus, Tsai (1925) found that his rats tended to choose food rather than an animal of the opposite sex when they had been deprived of both kinds of satisfaction But it is clear that, whether or not rats have preferences among possible mates, they have quite definite food preferences, as Young (1941) has extensively demonstrated Consequently, Tsai might well have obtained an opposite result by employing a food that was low on the rats' scale of preferences Likewise, remembering the curves from the obstruction method, it appears that the outcome depended also upon the deprivation periods, since, according to that method, the two drives do not come to a maximum at

the same deprivation time. These difficulties are encountered whenever the criteria of choice, sacrifice, or exchange are employed.

Many other criteria of motivational strength have been suggested. ***Amount*** or ***rate of learning*** and ***discrepancy between ability and achievement*** may be mentioned, with the comment that they are difficult to apply because they require control over many other factors such as fatigue, illness, distraction, and change of goal (cf. Thurstone, 1937).

A natural question raised by any such review of criteria as the foregoing is, what is the *best* criterion? or do all of these criteria measure the same thing equally well? To supply information relevant to this question, E. E. Anderson (1938*a*) obtained measures of the strength of the hunger, thirst, sex, and exploratory drives in a group of male rats, using 47 different criteria. These criteria included various measures of time, effort, and activity of the animals in a variety of situations: obstruction boxes of various types, locomotion and consumption situations, etc. Although the reliabilities of the measures were in general satisfactory, correlations among different criteria of the same drive were disappointingly low. Small but significant correlations appeared in comparisons among different criteria of the sex drive and among different criteria of the exploratory drive; but little or no intercorrelation appeared within the hunger drive and within the thirst drive. On the whole, then, the different criteria cannot be assumed to be criteria of the same thing. Which criterion (if any) is the best measure of each drive remains to be decided. The decision can probably not be made without achievement of a definition of strength of motivation that enables us to distinguish between fundamental and "accidental" criteria.

Further, if we accept for experimental purposes some one criterion, it is not difficult to obtain measures comparing the strength of the drive toward one goal object with that toward another. But even if both drives are at their maximum, general statements about the relative strength of the two drives are limited by the fact that particular goal objects had to be used in the comparison, and that one goal object, within a given drive, is more attractive than another. Thus, in comparing a given rat's number of crossings to

food and to a female, one has to use a certain kind of food and a certain individual female; this problem is not peculiar to the choice method. It seems necessary, then, to discover the most attractive goal object for each drive, as well as to bring the drive itself to maximum strength, if one wants to make a final comparison between the drives. Whether this reduces the problem to an absurdity, or whether a solution can be found, cannot yet be stated.

Production, Efficiency, and Incentives. The preceding discussion has been concerned with the question, what can we infer about strength of motivation from various observed measures of an organism's behavior? A question of equal theoretical and practical interest is, what alterations in behavior are to be expected if we modify the strength of motivation? Our discussion of this very broad question must be restricted to a few points.

Production and Efficiency.--If one takes as a criterion of strength of motive either the length of periods of deprivation, in the case of primary motives, or the addition or subtraction of incentive objects, in the case of both primary and secondary motives, a large literature exists to demonstrate that rate of learning, amount of retention, rate of locomotion, and rate of useful production vary with strength of motivation. This literature extends from early work such as that of Triplett (1898), through later studies such as those of Book and Norvell (1922) and a variety of experiments cited by Tolman (1932, pp. 39-83), to the recent work discussed by Hull (1943, pp. 226-257) and the experiments of Crespi (1942) and Zeaman (1949). A few generalizations from these data may be risked.

1. As motivation increases from zero to some moderate degree of strength, the organism's efficiency in attaining the goal to which it is motivated increases. This is nicely illustrated by Fletcher's (1940) study of the amount of work done by chimpanzees for various amounts of food, when the physical effort required was varied.

2. Efficiency has sometimes been found to decrease, as motivation increases beyond some optimal amount. An interesting example of this is Birch's (1945) finding that chimpanzees solved problems

less efficiently when under a food-deprivation of 36 or 48 hours *than after 6, 12, or 24 hours. Note should be taken of Lewin's (1938, pp. 101-102) discussion of "spread of tension" in connection* with such findings.

The "Yerkes-Dodson law" is an interesting result of the existence of an optimal amount of *negative* motivation. Yerkes and Dodson (1908) and Dodson (1915) used electric shock to punish their subjects (mice and kittens) for wrong responses in brightness-discrimination problems. Their results led to the generalization that the optimal strength of shock decreased as the difficulty of the discrimination was increased.

3. "Disruption" of behavior may occur when a goal is attained that is less desirable than what was expected (see the discussion in Tolman, 1932, Chapter IV). Crespi (1942) and Zeaman (1949) offer evidence that a change of incentive may have a "contrast effect": e.g., the efficiency of behavior to a reduced incentive may be less than would be predicted if the lower incentive had been employed originally.

4. The situations of reward and punishment, which are obviously of the greatest importance for the control of behavior, are too complex for discussion here. It is possible that offering an incentive to an individual who is already motivated in the same direction as the incentive (e.g., a bonus system in industry) may be treated as a simple case of added motivation. However, when a reward is offered, or a punishment is threatened, to induce someone to do something that he does not wish to do, an approach-avoidance conflict is brought about in the former case, and an avoidance-avoidance conflict in the latter case. These types of conflict situation are discussed in the section on Interrelations Among Motives, below. For a detailed treatment of reward and punishment, in which their implications for education and social control are made evident, see Lewin (1935, Chapter IV).

5. The general hypothesis that cognitive appreciation of the self and the environment, as gained from the processes of perception, memory, and thinking, tends to be influenced in the direction of active needs has recently been given much attention. We must con-

tent ourselves by referring the reader to *Interrelations between Perception and Personality: A Symposium* (1949), and the references cited therein. The fact that enthusiasm in this field of research has sometimes overridden the demands of technical caution might itself be taken as confirmation of the hypothesis.

Substitution

In spite of its evident importance, theory and research on substitution has made little progress. We summarize below a few conditions that have received some attention, largely at the hands of Lewin's students.

Conditions Affecting Substitution. *Motive Strength.*—Katz (1937) has observed that hungry animals will eat things that they will not eat when less hungry, and similar observations have been made by others. Generalizing, Lewin states that the realm of objects having a positive valence will increase as a need increases; he believes that this can be deduced from the principle that increase in need increases the valence of corresponding goal objects. It would be interesting to know whether the rank order of preferences for goal objects remains constant as the strength of a motive varies, but no data on this point are known to the writer.

Similarity of Goals and Acts.—Lewin's students (e.g., Lissner, 1933) have shown that resumption of an interrupted task, *A*, is less likely after performance of a similar task, *B*, than after a nonsimilar task, *X*. The interpretation is that Task *B* satisfies the need set up by Task *A* more than does Task *X*; therefore, Task *B* has more substitute value than Task *X*. However, this finding may mean merely that the experimenters were able to judge by the "similarity" of tasks that they would be related to the same need. Child and Grosslight (1947) report that fewer resumptions of a task occurred when it was followed by a task in which the same goal was reached by a different activity than when it was followed by a task in which a different goal was reached by the same activity. Hence, the similarity of activities may be related to their mutual substitutability only because highly similar activities are likely to have common goals.

Difficulty.—Lissner (1933) reported that success in a difficult task had more substitute value than success in an easy task. This is no doubt related to the phenomenon of level of aspiration, which is discussed below

"Reality."—Lewin asserted that behavior occurs at various "levels of reality" In the more real levels, boundaries between regions are stronger; in less real levels, boundaries are less rigid, and the structure of the situation is more readily affected by wishes and fears Actually putting together the pieces of a jigsaw puzzle would be more real, in this sense, than imagining oneself doing so Mahler (1933) had subjects solve problems at three levels of reality, by overt manipulation, by telling the experimenter how they were to be done, and by merely imagining the solutions She concluded that solutions at all three levels had some value in reducing the need for completion of the tasks, and that, in general, this value was greater for the more real levels Henle (1942) contends that all that Mahler could assert, under her conditions, was that completions that were more closely related to the subjects' actual goals had the more substitute value, since in fact the less "real" solutions were also those that Mahler herself recognized as the less closely related to these goals If a genuine experimental problem exists here, rather than a mere question of definition, it has yet to be solved

Structure of the Tension System Henle's (1942) aim was to discover principles that would enable substitute value to be *predicted.* She believed that Lewin's students were unable to make such predictions because they did not offer ways of determining the interrelations among tension systems The Gestalt principles of pair-formation under conditions of similarity and proximity were adapted by Henle to this purpose from their previous usage in perception, memory, and thinking Her conclusion was that substitute value is favored by conditions that produce pair-formation among the tension systems. It is not possible here to discuss alternative explanations of these results; it can only be remarked that Henle's stress upon the need for predictive principles in the field of substitution is clearly well taken.

Satiation and Co-satiation.—It is well known that under prolonged repetition, an act can become extremely unpleasant to the point that, at the extreme, the person will attempt to "go out of the field." During the course of this change, the act tends to be varied in one way or another and to lose in efficiency and precision, and it becomes more and more difficult for the person to attend to it. These are the familiar phenomena of "monotony." Karsten (1928), studying the problem in a motivational context, has called the process one of "satiation." The relation of this process to "fatigue" has never been clearly defined, but students of fatigue appear to be stressing its motivational aspects more than has been true in the past (cf. Hull's concept of "reactive inhibition" as an avoidance motive, section on Strength of Motives, above).

Here we are concerned, not with satiation itself, but with what Karsten called "co-satiation." She found that subjects who had become well satiated with a particular task were more readily satiated with certain other tasks than were originally nonsatiated subjects. For example, a child who has drawn so many pictures of a cat that he refuses to draw another is likely thereafter to draw fewer pictures of a dog than he would otherwise have drawn. However, this effect was shown to depend upon the extent to which the new task is seen by the subject as related to the old one. A person who has written his name so often that he asserts that he is physically unable to write at all may nevertheless cheerfully write an identifying signature on the back of the page at the experimenter's request; this identification is evidently not perceived as a continuation of the former task. If, as Karsten believes, co-satiation is determined by the structure of "regions within the person," it should be closely related to substitution. One might then expect that actions that are mutually co-satiating would also be mutually substitutable. Lewin (1935) seems to have assumed something of this sort in his theory of feeblemindedness.

Substitution in Psychoanalytic Theory.—A large part of psychoanalytic theory is concerned with expressions of motivation that has been displaced from its original aim. This is particularly true of such "mechanisms" as sublimation, transference, displaced ag-

gression, neurotic symptoms, etc. (cf. Henle, 1942, pp. 5 ff.). The development and operation of these mechanisms, and their place in normal and abnormal behavior are problems of "personality dynamics," requiring for their treatment much more than purely motivational concepts. Here we are able only to raise the question whether the activities that take place in identification, fantasy, sublimation, etc., are related *functionally*, or merely *historically*, to the motives that the clinicians find behind them. If a dream is the fulfillment of an unconscious wish, does the dream *satisfy* the wish? If so, genuine substitute value is possessed by the operation of these mechanisms. If not, two possibilities remain. First, the goals attained by the mechanisms may be no more satisfying than a stone that a hungry man mistakes for a loaf of bread; Lewin would call this a case of substitute valence without substitute value. Second, the clinician may be overlooking the motives that are now effective in the behavior because of his interest in uncovering the long history that has made the person what he is.

Distance, Barriers, and Frustration

Distance.– Common speech suggests that an important aspect of motivational situations is the distance between the person and the goal. "I came close to winning that game"; "I am a long way from getting my degree"; "So near and yet so far"; "That career is out of my reach"; these expressions all use the language of spatial measurement to describe what may be called "goal-distance." In some instances, goal-distance is highly correlated with spatial distance, as when a person is walking home from the railroad station, although it is unlikely that there is a linear relation between the two. However, a prisoner's distance from freedom may be measured by the time he has yet to serve; a student's distance from his degree by the number of barriers—examinations, courses, etc.—yet to be passed; a girl's distance from a proposal of marriage by her estimate of its likelihood. Furthermore, as the last example shows, we must distinguish between the "psychological distance" as it is perceived by the person, and the distance measured objectively.

The concept of goal-distance has been studied principally as a determiner of goal-attraction or rate of learning and locomotion. In his goal-gradient hypothesis, Hull (1943) supposes that, during the course of learning, those actions nearest the goal become most rapidly learned, and that this will predict or account for a considerable number of phenomena, including gradients of speed of locomotion that rise toward the goal, backward elimination of blind alleys in rats' maze learning, the selection of shorter alleys and the elimination of blind alleys, etc. The basis for the hypothesis is the belief that there exists a gradient of reinforcement such that the shorter the time between a response and its reinforcement, the greater is the resultant increase in habit strength. It will be noted that this hypothesis rests upon learning theory, and says nothing about the motivational aspects of the situation. On the other hand, Lewin and others have contended that the attractiveness or repulsiveness of a goal increases as it is approached; such a change in valence is dealt with by Lewin as a problem of motivation, rather than of learning. The discussions of Hull (1938) and Lewin (1938, pp 165-167) provide an interesting means of comparing their views.

A direct relationship between effort toward locomotion and nearness to either a positive or negative goal has been demonstrated quantitatively by Yale investigators (Miller, 1944), and has been employed by them in dealing with the theory of conflict.[6] Both Lewin and the Yale group have stated that the gradient falls more steeply away from a negative goal than from a positive one, and this appeared in the data cited by Miller. It will be of interest to determine whether this is quite general, or whether it depends upon such factors as the mobility of the goal and the kinds of behavior that are required in approach, as compared with escape, behavior.

Reversing the problem in an ingenious manner, Crutchfield (1939) has theorized that the greater the strength of motivation toward a goal, the greater will be the apparent goal-distance. In accordance with this hypothesis, rats trained to run a certain distance for food were found to run farther when hungrier, and less

[6] Note also Geier and Tolman's (1943) demonstration that rats become more active as they approach a goal, except toward the end of the path, where restlessness is supplanted by more controlled or "goal-directed" behavior

far when less hungry, under conditions in which speed of running seems to have been eliminated as a factor.

Barriers.—It is possible to distinguish obstacles that occur in the path toward a goal from a homogeneous goal-distance, by defining a barrier as a region in the path at which the difficulty of progress suddenly increases. However, Wright (1937), whose work on the effects of barriers constitutes the most extensive single treatment of the problem, has explicitly dealt with distances and barriers without differentiating between them.

Wright attempted to demonstrate that the presence of a barrier increased the attractiveness of an object which lay beyond it, in accordance with many proverbs of the type: "Grass on the other side of the fence is greener." Unfortunately, work on spatial, temporal, social, and other kinds of barriers (or distances) has led to no uniform result. Although Wright believed that he had demonstrated the increased attractiveness, others (e.g., Child and Adelsheim, 1944, and Irwin, Armitt, and Simon, 1943) have failed to confirm it. It should be noted that if no distinction is made between barriers and distances, Wright's conclusion is opposed by all the data that indicate that the gradient of attractiveness slopes downward from the goal, as discussed earlier in this section. The theoretical problems involved in either case are of considerable interest. Wright suggested that preferences for objects beyond barriers might be accounted for in various ways, such as the existence of "contingent properties" in the situation—attractiveness of the barrier itself, arousal of curiosity, concealment of defects, etc.; the damming up of energy by blockage of locomotion; or the satisfaction derived from successful coping with difficulty. Although their results were contrary to Wright's, Child and Adelsheim (1944) suggested several conditions that might lead to preferences for barred objects. For example, experience may have taught subjects that valuable objects are frequently difficult to attain, so that a barrier comes to be a mark of value. Whether or not there is some fundamental, "noncontingent," relation between attractiveness and barriers, or whether, indeed, this is itself a meaningful possibility, we are far from knowing at present.

Frustration. Theorists of the most diverse views, such as James (1890), McDougall (1923), Freud (1943), and Dollard *et al.* (1939), have asserted, each in his own language, that the blocking of goal-directed activity is likely to result in pugnacious, combative, attacking, or aggressive behavior. Maslow (1941) believes that a sharp distinction must be made between responses to deprivation and to threats to the self; similarly, Rosenzweig (1944) classifies responses to frustration as need-persistive or ego-defensive. In her observations on subjects interrupted in the performance of a task, Ovsiankina (1928) noted both the tendency to resume the task ("need-persistence") and hostile actions directed against the experimenter ("ego-defense"?), and a large amount of the work of Lewin's earlier students concerned itself with the results of such interruptions. Among such results, Lewin stressed the likelihood that a barrier will acquire negative valence (cf. the problem of the obstruction-box) and be subject to attack by the frustrated individual.

In their extensive study, Dollard *et al.* (1939, p. 11) defined frustration as "that condition which exists when a goal-response suffers interference," and aggression as "an act whose goal-response is injury to an organism (or organism-surrogate)," and proceeded to assert the hypothesis that "the occurrence of aggressive behavior always presupposes the existence of frustration and, contrariwise, that the existence of frustration always leads to some form of aggression" (p. 1). This hypothesis was then applied in detail to a wide range of individual and social phenomena, from the toilet training of the infant in our culture to the characteristic ferocity of the Ashanti. We may comment on a few of the many points of theoretical interest suggested by this work.

1. The basis of the hypothesis is left in doubt as beyond the scope of the investigation. Whether aggressive responses are innately associated with frustration or whether they are selected out of the child's early repertory of responses by learning is not decided.

2. The generality of the hypothesis makes it vulnerable to the demonstration of a single negative instance. It also results in classifying many actions as aggressive that do not, on the face of them, exhibit the destructiveness by which aggressiveness was defined; this means that care is needed to avoid circularity of reasoning.

Withdrawal to a less extreme position by Miller (1941) is indicated by his statement that frustration may lead to nonaggressive acts.

3. It is not clear how the hypothesis would deal with the extreme differences in responses to obstacles or privations according to whether or not they affect the "self" (Maslow, 1941).

4. Dollard *et al.* contend that aggressive behavior reduces the motivation toward aggression but does not reduce the motivation toward the original goal whose attainment has been blocked. Hence, aggression is not a form of substitution. On the other hand, they assert that the release of aggressive tendencies in destructive action is catharsis, in Freud's sense. The question arises whether these two views are consistent.

5. With the exception of the degree of generality ascribed to the relation between frustration and aggression, a very encouraging agreement exists among the treatments of the problem. As a single example, both Dollard *et al.* and Lewin emphasize the tendency toward imaginative or "irreal" responses to frustration in phantasy, verbal attacks, and other hostile but not directly injurious actions.

Among results of frustration other than aggression must be mentioned *regression.* While this term has sometimes meant the return to an earlier mode of adjustment (Freud, 1943), or to an earlier habit (O'Kelly, 1940), Lewin has used it to mean a return to a more primitive phase of psychological development, characterized by the breaking down of boundaries between the regions within the "life space," and the consequent simplification of its structure. This form of regression would occur whenever the tensions within the individual became strong enough to break down the boundaries between regions. Since, according to Lewin, an individual's development involves the gradual differentiation of the self from the environment, the greatly frustrated adult resembles in some ways a less mature individual—he is said to "act like a child." Barker, Dembo, Lewin, and Wright (1947) have also concluded that the individual's constructiveness ought to be lessened by this process and have presented data from experiments with children confirming this deduction. A good summary of work on regression has been written by Sears (1943, Chapter V).

Maier (1949) has presented evidence that certain compulsive, stereotyped, responses, which he calls *abnormal fixations,* may occur under extreme frustration. He argues vigorously that these fixations constitute a special type of behavior, and that they cannot be regarded as goal-directed.

Level of Aspiration

In Dembo's experiments on frustration (1931) subjects who were faced with difficult or impossible tasks were found occasionally to discard the goals imposed by the experimenter and to substitute for them objectives at a level of difficulty low enough to permit successful achievement. Dembo called this process a lowering of the "level of aspiration." Hoppe (1931) set the pattern for subsequent theory by asserting that experienced success and failure cannot be known from achievement alone, but depend upon the relation between the level of achievement and the level of aspiration that the subject has set for himself. With a low level of aspiration he may be content with "making a good showing," while if he sets his aim too high, no achievement will satisfy him. Furthermore, Hoppe pointed out that the immediate short-range goal of an action may be quite different from the "ideal goal" that the subject may hope to obtain in the long run.

One of the first facts to emerge from the experimental study of levels of aspiration is that subjects very commonly set their goals at a level somewhat higher than their previous performances can justify. As a result many failure experiences occur in a series of attempts. Lewin has called this the "paradox of level of aspiration." Since success is greatly preferred to failure, why does the subject not set his goals so low that he can be bathed in a constant atmosphere of success? Reflection immediately suggests that aspirations are influenced not merely by our immediate wants in a concrete situation, but also by what we think we ought to want. Social standards of various kinds set the value of goals and achievements, and also of persons who set such goals and make such achievements. The importance of such standards has been easy to demonstrate (e.g., Festinger, 1942). Indeed, from the theoretical point of view,

it is difficult to see how the phenomena of level of aspiration could arise at all in the absence of standards of value. One might interpret the work of Anderson (1940) as evidence that children begin to set genuine levels of aspiration as they become capable of valuing aspects of their performances other than the actual satisfaction or frustration of their immediate wants. It is significant that Anderson found a child's willingness to risk failure to be a mark of "mature" aspiration behavior. Thus, what seems at first to be paradoxical may turn out to be an essential feature of the phenomenon of aspiration

Attempts to develop a general theory of level of aspiration have regarded it as a situation in which there is a conflict of several motives (Frank, 1935; Lewin *et al*., 1944). Since achievements at a high level of difficulty are rewarded by the approbation of others, there is sufficient motivation to push aspirations upward. And even when this makes failure quite probable, it was found by Sears (1940) that subjects seemed to receive some satisfaction from demonstrating publicly that, whatever their performance, their aspirations were high. On the other hand, fear of failure should furnish motivation toward a relatively low level In addition, it is supposed that there exists a kind of "reality principle"— i e , a desire to be able to achieve whatever goal has been set, in order to be correct in one's predictions about oneself Lewin *et al* (1944) have attempted to make quantitative predictions of levels of aspiration by means of a theory of this kind. It seems likely that further advances toward quantification could be made by applying such a principle as Helson's (1949) "adaptation level" to the series of performances that precede the setting of a level of aspiration The process of setting a level of aspiration appears to have much in common with the process of formulating a psychophysical judgment (McGehee, 1940)

On the basis of such a theory, aspirations will be expected to show different degrees of "realism" Some will be largely determined by an objective cognitive appreciation of the situation; others will be affected considerably by the individual's wishes. Relatively realistic aspirations should be set relatively close to past performances, these being, under ordinary conditions, the most relevant data for prediction about future performances Unrealistic aspira-

tions, since they are determined by hopes and fears, would be expected to lie farther above or below the past achievement level. Such differences in discrepancy scores (i.e., differences between level of performance and level of aspiration) are easy to induce by experimental instructions (e.g., Preston and Bayton, 1941); and they appear even when the instructions are held constant (Frank, 1935; Gould, 1939; etc.). Irwin (1944) asserted that successive realistic expectations ought to be more highly correlated with the performances than with each other. On the other hand, unrealistic expectations ought to be relatively independent of performance but related to each other, as long as a given mood of caution, hope, or other affective state is maintained. These predictions were fulfilled in a number of sets of data upon which they were tested. This outcome is undoubtedly related to Frank's earlier finding of a greater "rigidity," i.e., less variability, among unrealistic aspirations.

Level of aspiration has been studied as a means of self-defence (Holt, 1946), and as a personality trait in normal persons, neurotics, and psychotics (Gould, 1939; Eysenck, 1947; Escalona, 1940).

The relation of aspirations to strength of motives is of theoretical interest. It has been suggested that the discrepancy between previous achievement and present aspiration might be taken as an index of strength of motivation; and Bayton (1943) was able, although with difficulty, to determine a positive relation between motivation and aspiration, when achievement was held constant. From the theory stated above, one might expect relatively little relationship to the mean height of aspiration, but a more marked relation to the variability of aspirations, since strong motivation may well increase fears as well as raise hopes. The converse problem, i.e., the use of the level of aspiration by the person as a self-motivating device, has also been studied (Holt, 1946).

Interrelations Among Motives

Physiological Interrelations.—There is reason to believe that the physiological antecedents or concomitants of some motivational states are opposed to those of others. Thus, it is a commonplace

that one's appetite may be destroyed by the receipt of bad news. To account for this, purely psychological terms might be used: e.g., one could suppose that the bad news had lessened the importance, or what Lewin (1938) calls the "potency," of eating; but this assumes that the appetite, though less effective in behavior, still exists. There are, however, physiological grounds for expecting a genuine reduction of hunger if fear or anxiety produces such disturbances as inhibition of gastric contractions and of peristalsis. At the level of experiment, E. E. Anderson (1938b) reported that measures of "emotionality" were inversely related to measures of sexual behavior in the rat. He suggested that the opposition of the sympathetic and parasympathetic nervous systems, of which the former was presumably active in "emotionality" and the latter in the sex drive, might be responsible for the difference in amount of sex activity of the two groups, but he favored an explanation in terms of the opposition of the thyroid and pituitary glands. His data cannot tell us whether emotionality inhibits the sex drive, or whether the reverse is the case.[7]

Different Means or Paths to a Single Goal.- The same goal can usually be approached in a variety of ways, even within the limits of the individual's repertory of knowledge and behavior. Therefore, in a given situation, he may have to choose between running and walking, between a book and a lecture, between speed and accuracy, etc., not to mention choices of a marriage partner or a vocation in the pursuit of long-range objectives such as a happy family life or economic security. Granted that more than one goal is in fact likely to participate in these situations, decisions would have to be made even in the case of a single goal, since different means and paths are often incompatible with each other. Deliberation over the most adequate way of achieving a purpose might well be difficult and slow. It should, however, be distinguished from conflicts that reflect the presence of two or more motives.

[7] We must leave for discussion elsewhere the differences between motives and emotions. Even if fear and anxiety are not to be thought of as motives, they commonly have escape or avoidance motives closely associated with them. We regret also that we cannot take space to discuss the combination of "relevant" and "irrelevant" drives (e.g., Kendler, 1945).

Two or More Goals in Different Directions.—Lewin regarded the concept of the direction toward or away from a goal as important enough to warrant the application of a special geometry, that of "hodological" space, to represent it. He treated this hodological space as a form of topological space in which paths from one region to another are distinguished (Greek *hodos* = a way or path). To obtain an adequate comprehension of his position, familiarity with his monograph (1938) is indispensable.

The common conception that any two paths or means are leading in the same direction if the individual can go toward the same goal by either of them will be used here. Restricting ourselves to but two goals, there can be distinguished (1) instances in which these two goals correspond to a single motive, and (2) instances in which the two goals correspond to two different motives.

(1) When a choice must be made between two goals that correspond to a single motive, we have the problem of substitution, which has been discussed in the section on Substitution above. Considerable difficulty will often be met in differentiating the choice between two goals from the choice between two means or paths to the same goal, although it should not be hard to find clear-cut cases of each.

(2) When goals corresponding to different motives are found in opposed directions, a situation of *conflict* exists.

Conflict. It is encouraging to discover that a considerable degree of agreement upon the general characteristics of conflict situations is to be found between such contrasting theorists as Lewin (1938), on the one hand, and Miller (1944) and other members of the Yale group, on the other. The elementary forms of conflict situations are indicated as follows:

Type I, approach-approach conflict: presence of two attractive goals, approach to one of which is incompatible with approach to the other.

Type II, avoidance-avoidance conflict: presence of two repulsive goals, escape from each of which is incompatible with escape from the other.

Type III, approach-avoidance conflict: presence of a positive goal, approach to which is incompatible with avoidance of a negative goal.

Two important assumptions are generally made: (1) that gradients of attractiveness and repulsiveness exist, so that both attraction and repulsion fall off as distance from the goal increases; and (2) that the gradient of repulsiveness is steeper than that of attractiveness. Evidence exists in favor of both of these assumptions The Type I situation can then be regarded as an instance of an *unstable equilibrium,* since if an individual is midway between two equally attractive goals, and if he moves toward one of them for any reason, that one will become more attractive, the other less attractive, and he will continue toward the goal in the same direction. On the other hand, both Type II and Type III would constitute *stable equilibria.* In Type II, movement toward one of the two repulsive goals would increase its repulsiveness and decrease the repulsiveness of the other, leading to a retreat, and in general, to oscillation around the point of equilibrium In Type III, in which an attractive and a repulsive goal are in the same direction (whether they be two objects or two aspects of the same object), movement toward the attractive goal increases its attractiveness but also increases more rapidly the repulsiveness of the negative goal; consequently, if at any point the repulsiveness is greater than the attractiveness, the individual will oscillate around a point of equilibrium at some distance from the two goals Furthermore, according to these principles, decision times should be longer in Type II and Type III situations than in Type I situations The experimental evidence for these hypotheses has been described by Miller (1944), to whose excellent survey the reader is referred for a more detailed treatment of the whole problem

It has been pointed out, especially by Lewin, that if the individual in a conflict situation makes a "sideways" movement that increases his distance from both goals, he will tend to be drawn back into the situation when both goals are positive (Type I), but will tend to continue "going out of the field" in the case of two negative goals (Type II). In the latter case, if one wishes to force the individual to make a decision, as when one threatens a child with pun-

ishment to cause him to do something distasteful to him, it will be necessary to enclose the situation with barriers—physical boundaries, prohibitions, etc.—to prevent his escape. These barriers, including the parent or other authority, are likely to be tested by the child and to acquire negative valence when they frustrate his attempt to free himself from the situation.

Cartwright and Festinger (1943) found it necessary to postulate the existence of a "restraining force" against making a decision in a conflict situation. On the other hand, common observation suggests that the presence of a prolonged conflict may produce forces leading to its resolution "by fiat," as it were, a more or less arbitrary decision may be made merely to escape from the state of indecision. An inability to resolve even trivial conflicts is observed in some forms of mental disorder.

While Lewin and Miller come to much the same conclusions on the general features of conflict, they arrive at these conclusions from quite different kinds of argument. Miller depends largely upon learning theory and the kind of hypothesis that is illustrated by the goal gradient, while Lewin makes deductions from the assumed momentary state of the individual in conflict, with little regard to the factors that have brought this state about. Actually Lewin (1938) offers two different attacks upon the problem of conflict. One of these deals with conflict situations simply as situations in which the individual is acted upon by two or more forces. The other, which he himself regards as more thoroughgoing, looks upon conflicts as cases of "overlapping situations." The person is conceived of as being in two or more situations simultaneously, each situation having a certain "potency" relative to the others. The concept of potency is analogous to that of "attention," but does not refer to "conscious clearness." By means of this concept it is possible to account for instances in which an individual acts toward a goal even when its attractiveness is less than that of some other goal. Thus, when dinner time arrives, the potency of the eating situation may become sufficiently strong to cause a person to interrupt an activity the goal of which is much more attractive than the food that he is about to eat. Whether this departure from theoretical simplicity is necessary or desirable remains to be seen, but it is

noticeable that theories that attempt to eliminate concepts like that of attention are likely to introduce them under another guise—"mental set," etc. (cf. Hebb, 1949). However, useful as such a concept may be, it always entails the necessity of determining the laws by which it operates, and the danger of using it for easy pseudo-explanations

Levels of Organization.—An abundance of clinical evidence, as well as some from the psychological laboratory, suggests that the adult human being's personality is not fully unified and integrated, but rather must be regarded as structured in various ways. A common concept is that of levels of integration, familiar to us at all stages of psychological investigation from the neurological levels of the physiologist and physiological psychologist to Freud's conscious, preconscious, and unconscious levels and his id, ego, and superego.

It appears that motives may interact, not only at a given level of psychological functioning, but also between levels Clinicians maintain that those conflicts which involve the "central region" of personality, the "self," are the only ones that are significant for mental health. According to Freud (1943), mental illness always implies conflict between two regions of the personality, such as the id and the superego. Maslow (1941) asserts that deprivation of sexual activity under a vow of celibacy is in no way as disturbing as a deprivation resulting from rejection by a loved one Further, the influence of Freud and other students of mental illness has led to enormous stress upon lack of insight into one's own motives as the basis for failure of conflicts to be resolved. Among experimental psychologists who are not psychoanalytically oriented, Lewin has perhaps given most attention to the development of structural ideas, with his notions of interrelated regions within the person, differences in the rigidity of boundaries between these regions, overlapping situations, and levels of "reality." The experimental study of relations between motives in different regions or levels of personality, and of the development of differentiation between "ego-needs" and more superficial needs, constitutes a most promising field for the coming generation of psychologists

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Suggested Readings

At present there appears to exist no adequate introductory survey of motivational theory. The references cited in this chapter have been chosen with consideration of their availability, their bibliographies, and their usefulness as introductory discussions. The following may be singled out for special mention:

History: Troland (1928).

Systems: Guthrie (1938), Holt (1931), Hull (1943), Tolman (1932), Lewin (1938, 1946), Freud (1938, 1943).

Primary motives: Morgan (1943), Freeman (1948).

Secondary motives: Allport (1937), Murphy (1947), Miller and Dollard (1941), Miller (1948b).

Strength of motives: Warden *et al.* (1931).

Substitution: Henle (1942).

Distance, barriers, frustration: Wright (1937), Dollard *et al.* (1939), Maier (1949).

Level of aspiration: Lewin *et al.* (1944).

Conflict: Lewin (1938), Miller (1944).

Had it not been seen too late, Hebb's (1949) stimulating book would have affected our treatment of a number of important motivational problems.

CHAPTER 6

FEELING AND EMOTION

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Introduction

Four main points of view can be distinguished in the modern theory of feeling and emotion: the introspective, typified by Wundt; the dynamic, typified by Freud; the behavioristic, typified by Watson; and the physiological, typified by Cannon. We shall deal with these four points of view in order. For each we shall dip into the past enough, but only enough, to bring out the trend of theory within the framework of the point of view. This means that the present chapter will be organized historically, but will not constitute a history of the subject. Such a history is already available elsewhere (Gardiner, Metcalf and Beebe-Center, 1937).

The place for any over-all conclusion is at the end of this chapter. To orient the reader, however, we shall present a thumbnail sketch of our conclusion here. The terms feeling and emotion, we shall find, are now considered to refer to concepts, not to data. This view was forced on introspective psychology by the finding that, as far as description of consciousness is concerned, feeling and emotion are meanings, not specific contents. It was forced on dynamic psychology by the inadequacy of conscious factors for the interpretation of conduct, and the consequent need of inferring unconscious motivating factors, including unconscious feelings and emotions.

This view was also forced on Behaviorism, but in a different way. It, too, failed to find any specific data—specific responses—to which the terms feeling and emotion could be applied. It could not, however, follow the other two schools in relating these terms to what appeared to be markedly subjective concepts. In consequence, it

either disregarded the terms or proceeded to construct concepts corresponding to them from its behavioral data.

This latter procedure has had a tremendous effect upon the theory of feeling and emotion. It has shown that the vague concepts represented by the layman's "notions" of feeling and emotion can be replaced by closely related concepts defined strictly in terms of experimental data—indeed, actually constructed from the data. It has thus opened the way for a truly empirical and objective theory of feeling and emotion

What direction will such a theory take? Its goal is not simply to define more closely the layman's concepts, but to determine the general principles and mechanisms involved in the behavior referred to by these concepts Detailed specification of a few concepts corresponding closely to those used by the layman will obviously be of service in bringing out more clearly than is possible at present the experimental problems that need to be attacked The solution of these problems may well show, however, that such purely affective concepts are entirely superfluous for the specification of the principles and mechanisms underlying affective behavior

Theories within the Framework of the Introspective Point of View

Feeling and Emotion as Conscious Contents.—The conception of feeling and emotion as conscious contents reached its greatest development in the psychology of Wundt (1920) Feeling, he taught, was one of two basic elements of consciousness (the other was sensation) Feelings were so varied that they required, for characterization, three descriptive dimensions, the dimensions of pleasantness-unpleasantness, excitement-depression, and strain-relaxation. Corresponding to the nature of feelings with respect to these dimensions were characteristic changes of strength and rate of pulse and breathing. Pleasantness, for instance, was accompanied by strengthening and slowing of pulse and weakening and acceleration of breathing. The bodily changes accompanied the feelings because the physiological counterpart of feeling was a spread of excitation from sensory centers to motor centers.

Emotion, for Wundt, was an interconnected series of feelings, accompanied because of summation, by marked changes in breathing and pulse and also by certain bodily movements. Sometimes emotion fades out gradually, sometimes it is terminated abruptly by a marked change in feeling and in ideational content. Such an abrupt change is a volitional act, and an emotion terminating with such an act is a volitional process. A typical volitional process is one where the emotion terminates in a volitional act which removes the source of the emotion. Although both feelings and sensory contents are involved in volitional processes, it is the feelings which are important as far as motivation is concerned. "All feelings, even those of relatively indifferent character, contain in some degree an effort towards or away from some end" (Wundt, 1902, p. 303).

The subsequent history of this general conception is largely one of failure. The motivational side of the doctrine was the first to suffer. James' theory of emotion, making of it the sensory awareness of bodily responses, deprived it indirectly of motivational importance (James, 1884). Titchener questioned the very possibility of hedonism, and flatly denied it as a fact:

> "We might ask how it is that a mental process can incite or deter, 'stamp in' this and 'stamp out' that mode of reaction. All that is necessary, however, is an appeal to the fact. Movement follows on suggestion, and the conscious aspect of suggestion may be pleasurable, unpleasurable or indifferent. . . . Thorndike, in 1905, declares that 'any mental state whatever may be the antecedent of an intentional act.' This position, extreme as it is, squares far better with the introspection of the reaction experiments than does the traditional doctrine of motivation by pleasure-pain" (Titchener, 1910, pp. 468-9).

Titchener still agreed with Wundt, however, that emotion consists largely of feeling, and that both are conscious contents. This part of Wundt's doctrine falls prey to subsequent experimentation. Conklin and Dimmick published in 1925 an introspective analysis of fear in which it was found that fear always involves unpleasant feeling, but that its truly emotional aspect is a meaning (Conklin and Dimmick, 1925). This supported Titchener's view that emo-

tion involves feeling, but closed the door to the study of emotion within his system, for according to him, "Science . . . does not deal with . . . meanings" (Titchener, 1917, p 26).

As to feeling, the result was much the same; in 1924 appeared the last major attempt to characterize feeling as a conscious content, Nafe's frontal attack upon the problem in Titchener's laboratory. The task of deciding upon the nature of feeling was placed squarely on the shoulders of the observers "When you judge that the feeling is at its maximum," they were instructed, "break off your observation and describe the feeling as accurately as you can" (Nafe, 1924, p. 510). The general conclusion was that "pleasantness, as a psychological experience, consists of discrete bright points of experience in the general nature of a thrill but usually much less intensive. It is vaguely localized in the upper part of the body and quickly adapts or fatigues Unpleasantness is similar but characteristically duller, heavier, more of the pressure type of experience, and is localized in the abdomen or in the lower part of the body" (Nafe, 1929, p 411)

The obvious possibility that Nafe's results were due to observer bias led Young to repeat the experiment with one of Nafe's own observers, and with two other observers who had a different psychological background The former observer confirmed Nafe's findings, the two latter did not Young's reaction to this result was clear and picturesque "In the search for data which are independent of training I see hope, not in further training of trick Os" (Young, 1927, pp 188-9) A later experiment from the Cornell laboratory by Wells (1930) confirmed Young's findings in showing that results such as Nafe's depend upon a specific attitude on the part of the observer

Granted that Nafe did not prove that pleasantness and unpleasantness *are* bright and dull pressures, his experiment indicated an interesting correlation between the variables hedonic tone and brightness-dullness of pressures A subsequent experiment of Hunt (1931) confirmed this correlation, as did one by Ruckmick (1935). An experiment by Converse (1932) confirmed the correlation for colors and for musical pieces, though to a lesser extent than had Hunt's results. In the case of tones, however, Converse found no correla-

tion whatever She concluded against any causal relation between pressures and feeling.

Hunt has interpreted his results not in terms of a theory making pleasantness and unpleasantness conscious contents, but in terms of one which makes them meanings. "These pressures are the complex sensory resultant of the general bodily adjustment involved in the affective response. In terms of the context theory of meaning, these pressures are the *sensory* core of affectivity." The verbal response may, however, become conditioned directly to the stimulus "Once we form such a verbal response, it may be used independently on a higher level, i.e., in the logical thought processes, without the accompaniment of any sensory determinant. Thus we may speak of pleasantness and unpleasantness without experiencing any sensory content" (Hunt, 1933, p 347).

Meantime, much the same conclusion had been reached, on other grounds, by Carr, and formulated as the judgmental theory of affection. "It assumes that pleasantness and unpleasantness are attributes which we ascribe to any stimulating situation in virtue of our normal reaction tendency toward it" (Carr, 1925, p 290) Situations that normally arouse a positive reaction, i e , one tending to enhance, maintain, or repeat the situation, are judged pleasant Situations which arouse negative reactions, i.e., ones which minimize or rid the organism of the situation, are judged unpleasant Situations which normally arouse neither positive nor negative reactions are regarded as lacking in affective tone

Carr points out that this theory differs from the attributive, sensory, and elemental conceptions in that it denies that "pleasantness and unpleasantness are constituent items of experience that can be analytically observed It assumes that pleasantness and unpleasantness are names for the two distinctive meanings which objects may acquire in virtue of the type of adaptive response which they normally elicit" (Carr, 1925, pp. 298-9)

Carr's theory has more recently been developed into the "judgmental theory" of pleasantness and unpleasantness by Peters. "The *sine qua non* of affection," writes Peters, "is the judgment, pleasant or unpleasant, which the individual makes of objects in the light of his knowledge of his own reaction tendency toward them. The con-

scious content need be in no way different from that involved in any other type of judgment" (Peters, 1935, pp. 356-357).

Let us summarize this section The history of the conception of feeling and emotion as conscious contents is one of failure. This failure becomes apparent at first in the field of motivation: action cannot be explained adequately in terms of such conscious contents. The death knell of the conception, however, is rung by the experiments seeking to specify these conscious contents. No such contents can be found

Interpretation of Psychophysical Data.—Meanwhile a good deal of information was being accumulated on the relations of affective dependent variables, such as hedonic tone, to a number of independent variables such as nature of the stimulus, set, or age of observer (Beebe-Center, 1932) Much of this work was unfortunately done without proper appreciation of the implications of multiple determination of affective judgments without recognition that any one relation between a dependent variable (e g., hedonic tone) and an independent one (e g., intensity of stimulus) was only valid for the particular values of the parameters involved in the situation This, however, is a weakness that has been characteristic of all of the development of psychology isolation of parameters arises largely from inability of earlier experimentation to control variability, and is thus a sort of *esprit d'escalier.*

A notable exception to this weakness is the work of Guilford (1934) on the relation of feeling to color By an ingenious statistical technique he was able to partial out from his data the role of hue, lightness, and saturation in determining affective value, and also to estimate their joint effectiveness in this respect Figure 6 1 presents the curve which he obtained for the relation of affective value to hue with lightness and saturation held constant

A related problem in connection with taste stimuli has been that of bringing together into a single system of coordinates the data on the relation of an affective dependent variable to concentration in the case of different solutions. Beebe-Center and Waddell (1948) have recently developed a general psychological scale of taste. Figure 6.2 makes use of this scale to present in a single system of

coordinates the classical data of Engel (1928) on the affective value of solutions of sucrose, sodium chloride, quinine sulfate, and tartaric acid. The dependent variable, percentage preference, is defined as 100 (P + ½)/(P + I + U) in which P, I, and U represent the number of judgments pleasant, indifferent, and unpleasant upon any given stimulus The independent variable is psychological taste intensity in terms of gusts, one gust being the psychological taste intensity of a 1 per cent solution of sucrose

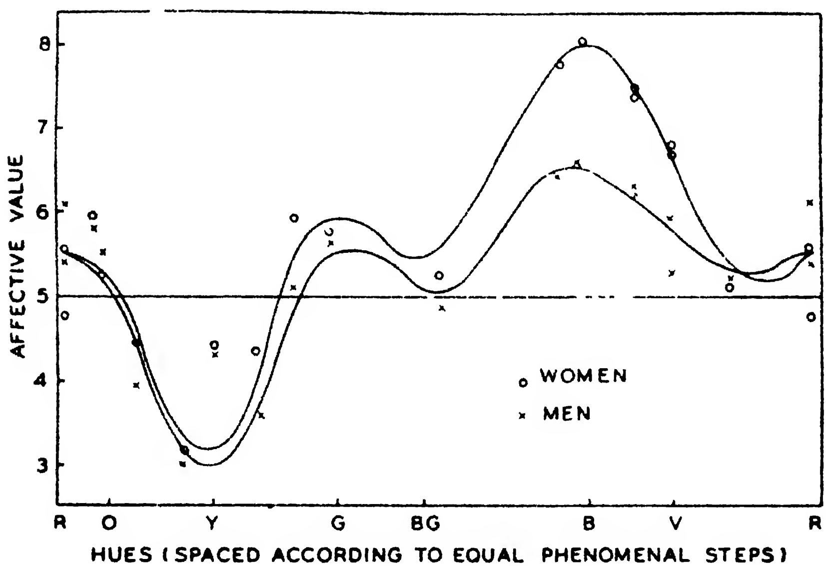

FIG. 6.1 Curves showing the relation between affective value and hue, for both men and women (From J. P Guilford, *J. exper. Psychol.*, 1934, 17, 342-70, by permission of the author and the American Psychological Association.)

The principal problem for us to consider, however, in connection with the data on the psychophysics of feeling, is that of interpretation. So long as feeling and emotion were considered to be conscious contents no problem of interpretation arose. Facts like those of Engel specified the relationship between stimulus and conscious content. But if feeling is not a conscious content, just what is being specified by such facts?

The answer seems to be: the constructs that the psychologist builds out of the facts. We have seen that the terms pleasantness

and unpleasantness, as used by observers, represent meanings. Such meanings are the concepts built up by the layman to interpret his world. The psychologist, seeking to interpret his facts, does essentially the same but does so wittingly and, in so far as possible, according to strict logical procedures. Such concepts he calls constructs.

We shall have more to say about this form of answer in our conclusion. Meantime let us consider what appears to be a much

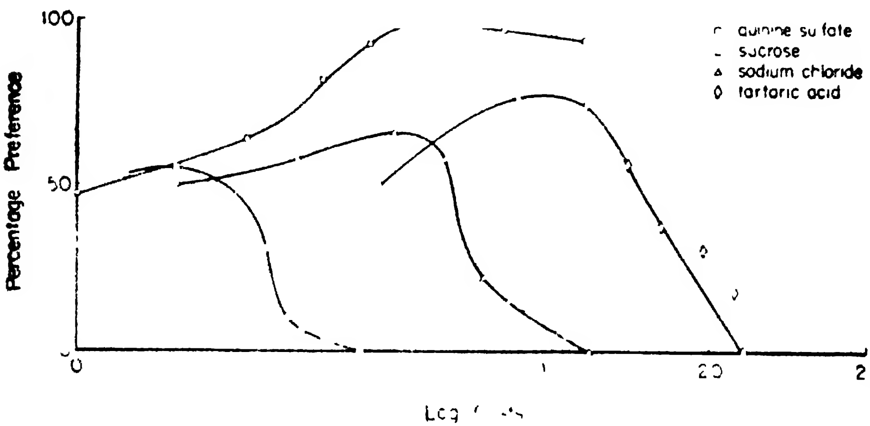

FIG. 6.2. Curves showing the relation between affective value and psychological intensity for solutions of sucrose, sodium chloride, quinine sulfate and tartaric acid. Affective value is specified in terms of percentage preference (for definition see text). Psychological intensity is specified in terms of gusts (for definition, see text). The basic data are from Engel (1928).

simpler alternative. In a recent statement of his views concerning feeling and emotion, Hunt (1948, p. 90) makes both correspond to "general reaction attitudes of the organism toward something in the environment." Accepting this view, it is obviously attitudes which are being specified by data such as those of Guilford and Engel. Acceptance of Hunt's view, also, we should like to add, provides a ready bridge between the affective psychology of the introspective school and the study of attitudes by social psychology.

Is this second answer, however, really different from the first? An attitude, too, is a concept. Its definiteness, too, will depend on the explicitness of its derivation from experimental facts. In the case of an affective attitude these facts will include such data as

those of Guilford and Engel. To say that such facts specify an attitude is no different, then, from saying that they specify the construct toward the generation of which they have contributed.

What about psychophysical data in the field of emotion? The problem as to what is being specified by such data has long been solved, in the Functional school, by defining emotion, not as conscious content, but as a state or function of the organism or of the individual. Good examples of this view are provided by the systems of Woodworth (1940) and of Carr (1925). It is essentially the view which is presented in what is probably, today, the standard textbook on emotion, Young's *Emotion in Man and Animal.* Young gives the following formal definition: "An emotion is an acute disturbance or upset of the individual which is revealed in behavior and in conscious experience, as well as through widespread changes in the functioning of viscera (smooth muscles, glands, heart, lungs), and which is initiated by factors within a psychological situation" (Young, 1943, p. 51).

Let us consider a little more closely Young's definition. The upset state of the individual is *revealed* by certain reactions to certain eliciting conditions. The state, then, is essentially defined by these relationships. Here again, the role of psychophysical data, as of any other data, is to specify the concepts constructed from them.

Theories within the Framework of the Dynamic Point of View

Freud.—The dynamic point of view is represented by a group of men in whom psychiatric training predominates. Their problem is to fathom the mechanisms of the organism that comes to them as a patient, in order to cure it. The symptoms it presents—conscious or behavioral peculiarities—are often impossible of interpretation in neural terms, but relatively easy of interpretation in terms of common-sense mental forces such as wishes, repression, conflict. In consequence their theories are essentially systems of such common-sense forces and processes.

The outstanding figure in this group is Freud (1920, 1922). His doctrine of feeling is essentially a form of hedonism. This hedonism

operates, however, according to two very different principles. According to the first, the so-called pleasure principle, the organism whose equilibrium is disturbed by a need hallucinates the desired object. This, Freud believed, occurs extensively in children. Such an organism further disregards unpleasant stimulation. This is illustrated in repression. Only when the pleasure principle has failed to yield satisfaction does the second principle, the so-called reality principle, begin to operate. According to this second principle, pleasure is sought within the limits of reality (not by hallucination), and present insecure pleasures are given up for later more secure ones.

Freud was not, however, a pure hedonist. To the two basic principles of motivation described above he added in 1922 a third, the repetition compulsion. Traumatic neuroses, he felt, could not be interpreted as wish fulfillments. They were, essentially, reinstatements of conditions causing pain. These reinstatements and also certain other forms of repetitive activity, Freud explained in terms of repetition compulsion, a principle antedating ontogenetically even the pleasure principle. Instincts are manifestations of the repetition compulsion, a tendency to the reinstatement of the original inorganic condition: "The goal of all life is death" (Freud, 1922, p. 47).

Furthermore, Freud's hedonism is not the truly basic factor in his doctrine of motivation. Rather, it and repetition compulsion both are manifestations of a more general biological function, that of "mastering and bringing to rest the mass of stimuli and the stimulating forces which approach it" (Freud, 1920, p. 309). Pleasure, he believed, is correlated with decrease of stimulation, pain with its increase. Thus hedonism is one mechanism for mastery of the impinging mass of stimuli. Another mechanism is the repetition compulsion, which allows the organism to develop a sort of immunity to a mass of stimuli.

Emotion, for Freud, involved essentially the same mechanism as hysteria.

> "An emotion, in the first place, includes indefinite motor innervations or discharges; secondly, definite sensations which, moreover, are of two kinds, the perception of motor activities

that have already taken place, and the direct sensations of pleasure and pain, which give the effect of what we call its feeling tone. But I do not think that the true nature of the emotion has been fathomed by these enumerations. We have gained deeper insight into some emotions and realize that the thread which binds together such a complex as we have described is the repetition of a certain significant experience. This experience might be an early impression of a very general sort, which belongs to the antecedent history of the species rather than to that of the individual. To be more clear: the emotional condition has a structure similar to that of an hysterical attack, it is the up-shot of a reminiscence. The hysteric attack, then, is comparable to a newly formed individual emotion, the normal emotion to an hysteria which has become a universal heritage" (Freud, 1920, pp. 342-343).

It is within this framework that Freud developed his doctrine of anxiety, the prototype, and still to a large extent the cornerstone, of modern dynamic views of feeling and emotion Anxiety is the acknowledgment of weakness of the ego, in the face of demands placed upon it There are three types of anxiety: objective anxiety, when the demands on the ego come from reality; neurotic anxiety, when the demands come from the id (essentially, the passions); and moral anxiety (guilt), when the demands come from the super-ego (essentially, the conscience). The two latter types, however, can be reduced to the former. Objective anxiety always involves a traumatic factor, a situation in which the organism is overwhelmed by sheer magnitude of excitation. In primary objective anxiety it is the occurrence of the traumatic factor which elicits the anxiety The birth trauma is chronologically the first instance of primary objective anxiety and all subsequent instances are patterned after it. In secondary objective anxiety the eliciting condition is not the occurrence of a traumatic factor, but the likelihood of its occurrence

Neurotic anxiety is essentially fear of consequences. The individual is unconsciously afraid of his libido, but this fear rests on fear of a traumatic factor which may occur as the consequence of libidinal action. In men, this traumatic factor is castration, in women withdrawal of care by parents. Neurotic anxiety gives rise to repression, a process similar to normal thinking, but unconscious. The ego,

unable to adjust to an impulse, hallucinates its satisfaction, but also the traumatic factor resulting from this satisfaction. Thereupon the pleasure principle represses the dangerous impulse.

Moral anxiety or guilt is also a derivative of objective anxiety As the individual develops, the super ego takes the place of parental authority, and moral anxiety takes the place of earlier objective anxiety in which the traumatic factor is loss of parental love or punishment. Freud did not, however, develop in detail his views of moral anxiety

McDougall. Only second in importance to Freud in the development of dynamic theories of feeling and emotion is William McDougall. His, however, is a negative influence Both made of motivation the principal problem of psychology Whereas Freud, however, based motivation largely on feeling and emotion, McDougall based it on instinct and gave feeling and emotion a very secondary role. The development of the theory of feeling and emotion within dynamic psychology has been very largely a battle between these basic points of view

For McDougall, feeling selects means, not ends "The instinctive impulses determine the ends of all activities and supply the driving power by which all mental activities are sustained; and all the complex intellectual apparatus of the most highly developed mind is but a means towards these ends, is but the instrument by which these impulses seek their satisfaction, while pleasure and pain do but serve to guide them in their choice of means" (McDougall, 1926, p. 45).

But even as a selector of means the role of feeling is secondary in McDougall's psychology Pleasure and displeasure are determined by successful and unsuccessful striving, they are, as it were, indices of such success or failure It is but a step from this view to one which would make feeling an epiphenomenon, and ascribe the selective effect to success or failure itself

As to emotions, they are frankly epiphenomena in McDougall's system. He believed that somesthetic experiences are an important factor in emotion, and agreed with James in regard to the mode of their arousal. The characteristic feature of emotion, however, is a

conative one, the experience of striving. Emotions are the awareness of instinct at work. They are the conscious by-products of instinct.

Anxiety in Modern Dynamic Psychology.—Three main features stand out in the development of dynamic theories of feeling and emotion. One, already referred to, is the defensive battle of the Freudian doctrine, assigning to feeling and emotion a major role in motivation, against encroachment by McDougall's doctrine of the primacy of instinct. The issue here is the very survival of dynamic theories of feeling and emotion. The second is the attempt to improve upon the conceptual structure of the Freudian doctrine and to extend its scope. The third feature, often combined with the second, but by no means always, is the attempt to provide a more solid factual foundation for dynamic theories of feeling and emotion.

With respect to the first of these features the battle has been clearly decided in favor of Freud. For one thing dynamic psychology has developed principally in circles concerned with practical problems of adjustment. In such circles the fact that feeling and emotion might be derivatives of instinct has been no bar to their use in systems of motivation. Nor has the possible limitation of their effectiveness to the selection of means, for adjustment is essentially just such a selection of means. To these two negative reasons should be added the positive one that Freud's doctrine of anxiety has proved an extremely powerful tool in understanding not only abnormal adjustment, but all forms of adjustment.

A good illustration of modern acceptance of anxiety as a fundamental factor in the psychology of adjustment is afforded by the following passage from Symonds' recent book *The Dynamics of Human Adjustment*.

> "This book consists largely of our exposition of man's methods of reacting to frustration. . . . The discussion will show that the most primitive reaction to frustration is aggressive behavior. . . . The person towards whom it is directed . . . is likely to respond by counter aggression in the form of punishment. . . . To the person who showed the original aggression, punishment is painful and something to be avoided. Consequently, any situation or behavior on the part of the aggressor which is likely to bring punishment on himself is anticipated by the arousal of anxiety.

Anxiety is one of the most distressing conditions in man and one which he does everything in his power to alleviate. . . . Individuals seek defenses against anxiety. . . . These adjustment mechanisms are modes of behavior which are to be found in everyone as methods employed to ward off anxiety" (Symonds, 1946, p. 3).

Another good illustration is White's anxiety theory of neurosis. It is anxiety, White believes, that has provided the bridge between the civilian neuroses originally studied by psychoanalysts and the neuroses studied in World War I, and also between the theory of neurosis of Freud and the theories of dissident psychoanalysts like Adler. In introducing his own anxiety theory of neurosis White writes:

"Motivation—especially unconscious motivation—and conflict—especially conflict among unconscious strivings—proved indispensable in understanding neurotic patients, but it was impossible to build up a satisfactory theory without considering the peculiar importance of anxiety and of the defensive measures used to hold anxiety in check. When Freud characterized anxiety as 'the fundamental phenomenon and the central problem of neurosis,' he summarized the impressions of many workers besides himself and the members of his psychoanalytic school" (White, 1948, p. 216).

Murray. We now turn to the second main feature in the development of dynamic theories of feeling and emotion, the attempt to improve upon Freud's formulation and to extend its scope. Freud accepted the view that psychology was the science of a particular class of entities—namely, conscious entities. Feeling and emotion, for him, were by nature conscious processes, even if they happened to be unobservable and thus unconscious. Furthermore, their nature was not a matter of inference but of direct inspection. These prejudices—for such they would now be considered—limited markedly Freud's freedom of action in matters of theorizing. Emancipation from them has removed this limitation for his successors.

A good example of the recognition that the theoretical concepts of dynamic psychology are constructs, not data, is afforded by Murray's system.

"Personality is the organization of all the integrative (regnant) processes in the brain. . . . The 'temporal pattern of regnant processes' is synonymous with the animal psychologist's 'temporal pattern of intervening variables'. . . . Since we know next to nothing about the electrical field of forces which constitute the physical aspect of the stream of consciousness, the best terminology available for conceptualizing each pattern of regnant processes is that which has been derived from introspection Words such as 'perception,' 'apperception,' 'intellection,' 'emotion,' 'evaluation,' 'need,' 'expectation,' and 'conation,' have proved most suitable, even for animal psychologists, provided it is understood that each of these stands for a physical process which can operate with or without self awareness Thus we can speak of unconscious perceptions, unconscious emotions, or unconscious expectations without the contradiction in terms which would be involved if we worked with the concept of consciousness instead of with the concept of the regnancy" (Murray and Kluckhohn, 1948, pp. 9-10)

In other respects, Murray's theory of feeling and emotion is closely related to that of Freud Affective factors play a tremendous role in motivation: "Personality operates to reduce 'dissatisfaction' and to heighten and extend 'satisfaction' " (Murray and Kluckhohn, 1948, p. 31). Whereas Freud, however, relates satisfaction and dissatisfaction to quantity of stimulation, Murray relates them to need-tension Furthermore, "it is not the tensionless state, as Freud supposed, which is generally most satisfying to a healthy organism, but the process of reducing tension" (Murray and Kluckhohn, 1948, p. 15).

Lewin.—Murray, it will be noted, retains the subjective emphasis of Freud. This emphasis has been largely dropped- though by no means completely, as we shall see in the Field Theory of Lewin (1946). Behavior, Lewin pointed out, is a function of both the environment and the person How can one get both the person and the environment into a single theoretical universe? By recourse to what he called the life space, in which both the person and the environment are represented in a topological (nonmetric) space. Lewin sought to specify the nature of the interaction of person and environment in this life space by means of a limited number

of constructs linked to observable facts by operational definitions and having clearly defined conceptual properties.

What is the status of feeling and emotion in Lewin's field theory? They are represented by the concepts valence and emotional tension. Valence, for Lewin, represents the attractiveness or repulsiveness of a region of the life space. "If the region *G* (which may represent an activity, a social position, an object, or any other possible goal) is attractive to the person, it is said to have a positive valence. . . . If the person is repulsed, we speak of a negative valence of *G*" (Lewin, 1946, p. 806). The strength of the force exerted on a person by a valence depends on the psychological distance from the person to the valence. Valence itself depends partly upon the nature of the goal *G*, partly upon the state of the needs of the person at the time.

As to emotion, it is represented in Lewin's system by emotional tension. Emotional tension is the outcome of conflict of opposing forces, and increases with the strength of these opposing forces. One of its simplest expressions is restlessness, usually perpendicular to the directon of the force to the goal. Increase of emotional tension makes "restructurization" difficult and leads to "primitivation" (regression). Ultimately it leads to an emotional outbreak characterized by the breakdown of normal inhibitions.

Psychosomatic Medicine.—So far we have considered only developments of Freud's view involving change. There has also been a very marked development in terms of scope. Freud applied his doctrine to the psychogenesis of functional disorders. Of late there has developed an interesting tendency to apply his and other dynamic doctrines to the psychogenesis of structural disorders. An outstanding figure in this movement is Franz Alexander (1936, 1947). In the second edition of his book *The Medical Value of Psychoanalysis* (1936) he reported a large-scale study of the personalities of patients suffering from various organic disorders, such as stomach ulcers, colitis, and asthma, and developed psychoanalytic hypotheses with respect to the psychogenesis of these disorders.

It is largely emotion which, in this movement, provides the bridge between psychological conditions and organic disorders. The de-

velopment of a more or less permanent state of emotion is inferred on the basis of dynamic theory, and the development of the organic disorder from this state of emotion is inferred on the basis of available physiological knowledge of emotion, such as that provided by Cannon and his school Indeed, White defines the field in terms of emotion. "Disturbances in which emotional maladjustment leads to chronic dysfunction in some organ system are nowadays referred to as psychosomatic disorders" (White, 1948, p. 423).

A direct demonstration of emotional genesis of gastric dysfunction is to be found in the work of Wolf and Wolff (1943). Observation of a subject with a large permanent gastric fistula showed that anxiety was accompanied by hyperaemia of the mucous lining of the stomach, and that such hyperaemia, in the presence of gastric acidity, resulted in small ulcer-like lesions.

Experimentation in the Framework of Dynamic Psychology.– We now turn to the third feature in the development of dynamic theories of feeling and emotion, the attempt to provide for them a more solid foundation in fact This attempt has been extremely fertile. Except for Cannon, no man has contributed as much to the experimental literature of affective psychology, directly or through disciples, as has Lewin (Lewin, 1946). It has also been extremely influential. Leeper imputes to experimental psychologists the view that emotion is disorganized response, in contrast with his own motivational view (Leeper, 1948) This imputation would have been correct twenty years ago It is not correct today. The pioneer experimentation of dynamic psychologists has won over experimental psychology as a whole to the motivational view of feeling and emotion

Specific dynamic theories of feeling and emotion have not, nevertheless, achieved widespread acceptance among experimentalists The reason seems to be that, although fertile in suggesting experimental problems these theories do not lend themselves well to strict connection with experimental facts. Sears states:

> "It seems doubtful whether the sheer testing of psychoanalytic theory is an appropriate task for psychology. Its general method is estimable but its available techniques are clumsy. In-

stead of trying to ride on the tail of a kite that was never meant to carry such a load, experimentalists would probably be wise to get all the hunches, intuitions, and experience possible from psychoanalysis, and then, for themselves, start the laborious task of constructing a systematic psychology of personality, but a system based on behavorial rather than experiential data" (Sears, 1944, p. 329)

The same is true, although to a lesser degree, of Lewin's system Let us consider one of its specific propositions relevant to the universe of feeling, namely the proposition that "The strength of a force toward or away from a valence depends on the strength of that valence and the psychological distance between the person and the valence" (Lewin, 1946, p 806) How does one measure "force toward or away from a valence"? Lewin's operational definition for force is for "resultant force," not for an individual force such as that toward a valence Furthermore, the operational definition itself is ambiguous Lewin writes "The combination of a number of forces acting at the same point at a given time is called the *resultant force* The relation between force and behavior can then be summed up in the following way Whenever a resultant force (different from zero) exists, there is either a locomotion in the direction of that force *or a change in cognitive structure equivalent to this locomotion*"[1] (Lewin, 1946, p 805).

How does one measure "psychological distance"? Fajans, whose work underlies that part of the proposition relating to psychological distance, writes: "If one does not wish to make the very inadequate identification of the psychologically relevant distance between person and valence with its physical distance, there can be no question of measurement of distance, at least for the time being" (Fajans 1932, p. 217) She then proceeds to use physical distance as the independent variable of her experiment!

Actually, Fajans has given us an extremely interesting study of the relation between the independent variable, physical distance, between child and doll or rattle lying before it, and a number of very molar dependent variables representing the duration of various

[1] Italics by present author.

forms of "turning toward" and "turning away." Only, however, insofar as Fajans and her readers are willing to accept her independent and dependent variables as representative, respectively, of the constructs psychical distance and force, does her experiment have any bearing on the proposition stated above

THEORIES WITHIN THE FRAMEWORK OF THE BEHAVIORISTIC POINT OF VIEW

Watson. – James' theory of emotion, extended by Wooley (1907) to cover feeling, was an open invitation to a theory of feeling and emotion such as that of Watson If emotion and feeling were awarenesses of bodily responses, specific emotions and feelings implied specific responses to be aware of. If one wished to eliminate consciousness from one's psychology, what would be more natural than to term these specific responses the emotions and feelings? Lange, for whom consciousness was an epiphenomenon, very nearly did so. Watson actually did so.

It is in connection with feeling -termed by him affection —that Watson first stated his general position regarding both feeling and emotion (1914). How can the Behaviorist, he asks, handle the problem of affection? By treating it, he answers, as a form of instinctive behavior. Every object, he believes, calls out, besides an overt response of the striped musculature "a definite and complex group of reflex activity in the erogenous zones" (1919, p 250) This reflex activity generates impulses of two kinds, impulses connected with tumescence, and leading ultimately to seeking movements, and impulses connected with detumescence and leading ultimately to avoidance movements. It is this reflex activity of the erogenous zones that constitutes what nonbehaviorists call the bodily substratum of affection.

Watson's view of emotion is essentially a generalization of this early position concerning feeling Basically, for Watson, "An emotion is an hereditary 'pattern-reaction' involving profound changes of the bodily mechanisms as a whole, but particularly of the visceral and glandular systems. By pattern-reaction we mean that the separate details of response appear with some constancy,

with some regularity, and in approximately the same sequential order each time the exciting stimulus is presented" (1919, p. 195).

Three such pattern-reactions can be distinguished in the human infant: fear, rage, and love. In the case of each, Watson describes the eliciting conditions and the responses involved in the pattern. In the case of rage, for instance, Watson writes: "Observation seems to show that the *hampering of the infant's movements* is the factor which, apart from all training, brings out the movements characterized as rage. If the face or head is held, crying results, quickly followed by screaming. The body stiffens and fairly well-coordinated slashing or striking movements of the hands and arms result. The feet and legs are drawn up and down; the breath is held until the child's face is flushed" (1919, p. 200).

This basic formulation, however, fits only the more stereotyped forms of emotional response such as anger and fear. "When we take into account the whole group of phenomena in which we see emotional manifestations in adults, a pronounced modification is necessary. Apparently the hereditary pattern as a whole gets broken up. At any rate, it largely disappears (the parts never disappear) except under unusual conditions, and there can be noted only a reinforcement or inhibition of the habit and instinctive (exaggerated and depressed reflexes, for example) activities taking place at the moment" (1919, p. 197).

What has happened, Watson believes, is that "In adults environmental factors have brought about the partial inhibition of the more external features of the primitive type of emotion. The implicit, mainly glandular and smooth muscular side of the pattern, remains. The emotionally exciting object releases important internal secretions which, without initiating new (part) reactions, reinforce or inhibit those actually in progress" (1919, p. 198).

Subsequent experimentation has failed to establish, for human beings, the specific emotional responses implied by Watson's theory. Landis (1924) subjected observers to strong emotional stimuli, photographed their facial expression and had them report on their emotional states. He found little correlation between the facial expressions and either the emotional stimuli or the verbal reports

of emotion. Sherman (1927) subjected very young infants to "emotional stimuli," including dropping and restraint. Observers were asked to judge the emotions displayed by the infants. As long as the observers were ignorant of the stimulating conditions there was little correspondence between the emotions which they named after watching the behavior and the emotions which should have been elicited by the stimuli according to Watson's theory. The outcome of these and other experiments has recently been summarized briefly and succinctly by Hunt: "No clear and univocal expressive patterns have been found for the different emotions" (1948, p. 97). The only exception which he recognizes to this statement is the startle pattern studied extensively by Landis and himself (Landis and Hunt, 1939).

In the case of nonhuman beings, the question of the existence of specific emotional responses is still an open one. Morgan (1943), basing himself on the studies of Cannon and Bard, recognizes the existence in cats and dogs of at least the three basic patterns distinguished by Watson—namely, rage, fear, and pleasure. Yerkes (1943) has published extremely convincing photographs of specific emotional expressions in chimpanzees. On the other hand, Hebb as we shall see further on, found it impossible to correlate specific responses to specific emotions in the case of chimpanzees (Hebb, 1946*a*).

Tolman.—How shall the behaviorist deal with feeling and emotion if there are no specific emotional responses, at least in man? A first answer of behaviorism to this question is provided by the system of Tolman. His view of feeling and emotion is sketched in his article "A new formula for behaviorism" (1922), described in general terms in his article "A behavioristic account of the emotions" (1923), and given detailed systematic formulation in his book *Purposive Behavior* (1932).

Emotion is not conscious content: "When an introspecting organism reports that he is angry, what you thereby know and identify is not his conscious state as such (which never gets into you) but rather that he is in a condition which makes him likely to behave in a certain way" (1923, pp. 218-219). Nor is emotion

behavior: "It is not the actually exhibited behavior, as such, which constitutes, behavioristically speaking, the emotion, but rather the *readiness* or *drive* for such a behavior . . ." (1923, p. 219).

"Feelings and emotions are . . . immanent determinants of behavior" (1932, p. 258). By immanent determinants Tolman means "functionally defined variables which are the last step in the causal equation determining behavior" (1932, p. 19). In the case of both feeling and emotion the immanent determinants involve three phases: expectation of weal or woe, consequent anticipatory responses, and sensory consequences of these responses.

In the case of feeling, the expectations of weal or woe are extremely general. Pleasantness, for instance, is an expectation that certain activities will lead to physiological quiescence, but it specifies neither means for reaching the quiescence nor the type of quiescence to be reached (*e.g.*, hunger satisfaction or sex satisfaction). Furthermore, feelings play but a small role in motivation. "Pleasantness and unpleasantness are, as we see it, results, not causes. They are indicators of cognitive expectations already made" (1932, p. 263).

The selective action ascribed to feeling in a hedonistic system is largely ascribed in Tolman's system to "demands for" specific objects akin to Lewin's valences. This variable is introduced to describe the fact that for a single physiological drive state (*e.g.*, hunger) different goal objects will yield different performances. Although these "demands for" are selective factors within the universe of a single physiological drive, their strength depends on the strength of the drive.

Hull. Behavior Theory (Hull, 1943, p. 21) achieves its clearest definition and most thoroughgoing exemplification in Hull's book *Principles of Behavior*. Here, better than elsewhere, we can understand what happens to the theory of feeling and emotion in Behavior Theory.

Hull's universe of data is, like Tolman's, molar organismic behavior. Hull's theoretical universe, again like that of Tolman, involves "presumptive intervening variables . . . theoretical constructs" (1943, p. 382). Hull's theory of behavior, and here he is not like Tolman, involves a very strict logical organization in postu-

lates and theorems, but this difference is more a matter of degree than of kind. In respect to formal features, Hull's theory of behavior is similar to that of Tolman.

Not, however, with respect to the kinds of theoretical constructs. Tolman's are taken largely from introspective psychology, although defined objectively. Hull's are not only defined objectively, but stem in the main from objective psychology and physiology

What, now, of feeling and emotion? They do not figure, *as such*, in his system at all The two words do not even appear in the index. This means simply that for Hull there is no need of distinguishing emotional behavior nor behavioral preferences from any other kind of behavior, nor does there appear to be any need to use *feeling and emotion* as constructs to explain behavior As far as general principles are concerned, the theory of feeling and emotion is simply Behavior Theory

This does not mean however, that there could not be within Hull's system a theory of feeling and emotion, just as there are physiological theories of respiration or digestion within the general system of physics In fact, Hull at the close of his book, writes: "It is to be hoped that as the years go by, systematic treatises on the different aspects of the behavior sciences will appear" (1943, p 399) He then lists a number of subjects the theory of which would be covered by such volumes One of them is "aesthetic values and valuation"

Nor does it mean that none of the constructs in Hull's system is closely related to feeling or emotion Quite the reverse is true with respect to his concept of reinforcement Historically, this concept stems directly from hedonism Classical hedonism, as represented by Freud, considered the effect of feeling on action to be direct This was a hedonism of the present, to use Troland's (1926) terminology. A very different form of hedonism a hedonism of the past, *for Troland* - was advanced in 1905 by Thorndike. It is a general law of behavior, he stated, that "Connections between neurons are strengthened every time they are used with indifferent or pleasurable results and weakened every time they are used with resulting discomfort" (Thorndike, 1905, p. 166). This general law, he pointed

out, includes the action of two factors, frequency and pleasurable result. The isolated action of the latter factor was then restated in terms of satisfaction and called the law of effect. It is this law of effect which is the direct antecedent of modern conceptions of reinforcement such as that of Hull (Hull, 1943, p. 80; Postman, 1947).

For the theory of feeling and emotion, however, it is not the close relation of Hull's concept of reinforcement to feeling which is most important. Rather, it is the fact that his system encompasses emotional and affective behavior without using feeling or emotion as constructs. This means that in Behavior Theory feeling and emotion may be used as constructs—witness Tolman—but do not have to be so used. Putting it more forcefully, in Behavior Theory the theory of affective and emotional behavior may involve neither feeling nor emotion.

This point is of tremendous importance if we are to understand the real unity that underlies such apparently disparate theories of feeling and emotion as the introspective one of Wundt, the dynamic one of Freud, the behavioral one of Hull, or the physiological one of Cannon. They are simply alternative *models* for the description of what is fundamentally the same factual universe. The one that is "truest" will be the one that best fits the facts. But all will be somewhat "true," for all fit some of the facts.

The point is also important for the evaluation of arguments like those of Meyer and of Duffy denying the usefulness of the concept emotion in psychology. In an article entitled "That whale among the fishes—the theory of emotions" Meyer states his view picturesquely as follows in the opening paragraph: "The whale has a twofold distinction among the fishes: first, when seen from a distance, it looms large among them, and second, on close examination, it is found to be no fish at all. Something like that I predict for the theory of emotions among the theories in psychological textbooks and periodicals" (Meyer, 1933, p. 292). His principal argument is that states termed emotional differ from those termed unemotional merely in degree, not in kind, and consequently that use of the term is both unnecessary and arbitrary. Duffy's (1934) article

-- much better organized, although less humorous than that of Meyer --comes to essentially the same conclusion on essentially the same grounds. In a later article she reiterates the view, but ruefully adds that it has had little effect. She writes: "But, alas, the concept, 'emotion' has not been abandoned" (Duffy, 1941, p. 283).

We have seen that this last statement is no longer correct: Hull, as far as fundamental constructs are concerned, has indeed abandoned emotion. We have also seen, however, that there is no reason why emotion should not be used as a concept if it helps provide a useful theoretical system--as it does in the case of Freud. Nor is there surely any reason not to characterize a certain universe of data by the terms "affective" or "emotional"; this custom is inherent in our language. The only step to avoid--and this Hull's system teaches us to avoid--is the inference that because common sense reifies emotion, the psychologist must do likewise. He may--as Freud does--or he may not, and follow Hull.

Skinner. A good demonstration of the possibility of using the construct emotion in psychology with a strictly operational definition is the Behavior Theory of Skinner (1938). The basic concept of Skinner's system is the reflex, a lawful relation between stimulus and response. The state of a reflex at any time, e.g., its threshold, is termed by him reflex strength. Skinner's psychology is to a considerable extent the study of the effects upon reflex strength of various operations, i.e., manipulations of experimental conditions. One set of operations which influences the strength of reflexes is the presentation of so-called "emotional" stimuli. The characteristic feature, here, is that there is change in strength not of one reflex alone but of a group of reflexes. "Other operations which induce an emotional change in strength are the restraint of a response, the interruption of a chain of reflexes through the removal of a reinforcing stimulus, the administration of certain drugs, and so on" (1938, p. 24).

As the changes in strength involve a group of reflexes it is necessary to deal with "*the* emotion as the 'state' of a group of reflexes. This is done by introducing a hypothetical middle term between the operation and the resulting observed change" (1938,

p 24). Emotion, then, is a changed state of the reflex strength of a group of reflexes as a result of certain specified operations.

As with Hull's *Principles of Behavior*, so with Skinner's *The Behavior of Organisms* the term feeling is not to be found in the index As with Hull the concept of reinforcement takes over much of the function which feeling has in a hedonism of the past Unlike the system of Hull, however, that of Skinner ascribes to its concepts certain of the functions of a hedonism of the present like that of Freud This occurs in connection with Skinner's concept of drive

Drive, for him, is akin to emotion in being a construct representing change in strength of a group of reflexes Drive and emotion are distinguished by the operations involved in eliciting the change In dealing with hunger, Skinner writes 'Hunger is not dual but multiple We must specify the food before we may predict the strength of the behavior of eating it and hence before we may assign a degree of hunger to the organism At any given moment each form of food commands a certain strength of behavior and all foods may be ranked in order according to their corresponding strength In extreme states of hunger the organism will eat practically anything, although it will still eat different substances at different rates In complete states of satiation it may eat nothing In any intermediate state it will eat all foods up to a given point in the *order of preference* . We define a sub hunger of this sort in terms of the co-variation of relative strengths We say that an organism is salt-hungry if the strength of behavior in eating all salty foods is relatively high' (1938 p 369)

Here we have an alternative conceptualization for selective behavior which in a hedonism of the present would be ascribed to the effects of feeling Although Freud was not concerned with such problems, he would presumably have assumed a single "hunger" and explained the differential reaction to various foods in terms of different feelings aroused by them Skinner explains the differential reaction by different sub hungers We shall return to this view in discussing the work of Richter and Young

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[2] Italics by present author

Estes.—A good illustration of the application of Skinner's systematic views to a concrete problem of affective psychology is provided by the work of Estes on punishment. In his book published in 1938 Skinner had reported an exploratory study indicating that the effect of punishment is to suppress a response temporarily rather than to eliminate it permanently (Skinner, 1938, pp. 151-160). Estes, using Skinner's general technique, confirmed Skinner's findings both for mild and for strong punishment (Estes, 1944). The immediate effect of punishment, he found, is to depress the strength of a response. In subsequent extinction trials responses that have been mildly punished show a compensatory increase in strength, so that the total number of unreinforced elicitations necessary for complete extinction is not altered. This compensation is only partial in the case of strong punishment, so that a saving can be effected in the total number of unreinforced elicitations necessary for complete extinction. Neither for mild punishment nor for strong punishment, however, is it possible to effect a saving in the total time necessary for complete extinction.

Meantime, Estes and Skinner (1941) had published an investigation of the effect, on strength of response, not of a shock alone, but of a shock preceded for a matter of minutes by the continuous sounding of a tone. This technique, they believed, corresponds to what is usually called the arousal of anxiety. Although on first presentation neither the tone nor the shock had any marked effect on strength of response, with repetition this strength was markedly depressed between the beginning of the tone and the occurrence of the shock. Estes and Skinner point out that in this situation the shock is not a (negative) reinforcement in the usual sense of this term, because it does not occur in strict conjunction with the response being depressed.

It is this latter feature which is stressed by Estes in discussing the theoretical implications of his work on punishment. In this study, too, he found no evidence that the correlation of the punishment with the response was important. He concludes that the term reinforcement should be reserved for stimuli which represent rewards. Noxious stimuli do not 'negatively reinforce' a response,

they arouse competing reactions: "A disturbing or traumatic stimulus arouses a changed state of the organism of the sort commonly termed 'emotional' . . . In addition to a generalized emotional reaction, a disturbing stimulus usually arouses a withdrawal response" (Estes, 1944, pp. 36-37).

Dollard, Miller and Mowrer.- The men we shall now consider represent, in a sense, a return to the position of Watson. For them, too, specific responses are key factors in emotion. Unlike Watson, however, these men ascribe to emotion a tremendous influence on conduct In this respect, they may justly be considered to be modern representatives of the Freudian view.

Mowrer extends the law of effect to cover not only organic pressures (needs) but the anticipation of such pressures. This extension, he feels, is consistent with what he believes to be "the growing tendency to eliminate the distinction between learning through 'punishment' and learning through 'reward.'" Anxiety, for Mowrer, "is a learned response, occurring to 'signals' (conditioned stimuli) that are premonitory of (i.e., have in the past been followed by) situations of injury or pain (unconditioned stimuli)." Furthermore, he holds, "Reduction of anxiety may serve powerfully to reinforce behavior that brings about such a state of 'relief' or 'security'" (Mowrer, 1939, pp. 562-4).

In 1940 Mowrer published an experiment designed "to test the hypothesis that anxiety-reduction acts as a reinforcing state of affairs (as defined by the law of effect)" (1940, p. 515) Irregular shocking, he found, resulted in slower escape learning than regular shocking. The former, he believed on other evidence, involved less anxiety reduction than the latter.

In the following year Miller and Dollard, in a book on social learning, propounded a view of emotion closely related to Mowrer's view of anxiety (Miller and Dollard, 1941). Emotion involves a learned response which acts as a drive stimulus. It is thus an acquired drive. It motivates random behavior, and, when suddenly reduced, it serves as a reinforcement of the preceding response More recently, Miller has published evidence in support of this view for the special case of fear (Miller, 1948). Rats, he finds, sub-

jected to shock in one compartment of a two compartment unit, will not only subsequently—without shock continue to escape, but will learn to press a lever permitting this escape, although no objective reward is provided.

Similar interpretations have been advanced for attitude by Doob (1947) and for effort by Solomon (1948). We thus have here an hypothesis which transcends the field of feeling and emotion in the ordinary sense of these terms Feeling and emotion become special cases of the operation of a very general feed-back mechanism, as suggested by Wiener (1948).

Hebb. - The introspective work of Conklin and Dimmick can be interpreted as a demonstration that emotion, in its common-sense usage, is a reification—a common-sense construct A similar demonstration from the behavioral point of view is afforded by the work of Hebb This work grew out of a striking inconsistency in a large-scale study of temperamental differences between chimpanzees at Orange Park "A formal experiment was set up to provide records of the actual behavior of the adult chimpanzees, and from these records to get an objective statement of the differences from animal to animal. All that resulted was an almost endless series of specific acts in which no order or meaning could be found. On the other hand, by the use of frankly anthropomorphic concepts of emotion and attitude one could quickly and easily describe the peculiarities of the individual animals, and with this information a newcomer to the staff could handle the animals as he could not safely otherwise" (Hebb, 1946a, p. 88).

Hebb rejects the view that emotion is a form of conscious content or of behavior For him the term "designates certain neurophysiological states, inferred from behavior, about which little is known except that by definition they predispose towards certain specific kinds of action" (1946a, p. 89). How, now, are such states inferred? This is the problem studied by Hebb in the particular case of recognition of emotions in chimpanzees by human observers. He extends his conclusion to recognition of emotions in man.

What are these conclusions? Hebb states the principal ones as follows:

"(1) The recognition of a full, characteristic expression is the classification of a deviation of behavior from an habitual base line. It is not a discrimination of the momentary behavior itself, but of the direction of the deviation, so that both present and past behavior affect the observer's judgment.

"(2) The recognition of emotion otherwise is a discrimination of a state of changed responsiveness detected from 'associated signs' acts which would not have a definite emotional significance in themselves, but which have been observed as the accompaniment of more openly emotional behavior

"(3) The emotions thus detected are inferred special states which facilitate or actually produce the primary emotional behavior of (1)" (1946a p 104)

Hebb further analyzed his data in an attempt to determine the nature of fear, considered as an inferred neuro-physiological state From this analysis, and from consideration of data in the literature, he developed an hypothesis the essential features of which are as follows:

The immediate source of fear is a disruption of a coordination, principally acquired in the timing of cellular activities in the cerebrum The disruption may be due to conflict sensory deficit or constitutional change With disruption there at once occur processes tending to restore the integration of cerebral activities, in fear these include either liminal or subliminal activation of processes determining avoidance Optimally avoidance tends toward completely averting the cerebral disruption and at this stage avoidance without fear would be said to occur" (1946b, p 271)

Richter. So far, in this section we have been concerned in the main with emotion We now turn to an experimental universe which bears directly upon the problem of feeling This is the work on the self-selection of foodstuffs, which centers largely around Richter, and the work on food preferences, which centers largely around Young.

As far back as 1921 Pearl and Fairchild showed that chickens given a large variety of foods to choose from, showed better growth than when given a rigid and supposedly adequate diet (Pearl and

Fairchild, 1921). Davis (1928) showed that children, given a large choice of foods, grew normally and improved in health. Harris, Clay, Hargreaves and Ward (1933) showed that rats, deficient in Vitamin B, selected from alternative diets the one containing that vitamin.

These investigations all involved natural foods, of composite and ill-defined make-up. Richter, in a series of brilliant experiments, has studied the nutritive selection of rats when the alternatives are not composite foods, but specific nutritive constituents, such as salt, iron, fat, protein. A first outcome has been the demonstration that under his experimental conditions the animals select nutritive constituents in accord with their bodily needs for these constituents —that their eating behavior appears to follow the general principle of homeostasis (Richter, 1942).

A good illustration of this work is afforded by an early experiment on adrenalectomized rats. Normally, such rats die in 10 or 15 days after the operation, apparently because of the excessive loss of salt. Richter found that when given free access to salt solutions, adrenalectomized rats voluntarily took a sufficient amount to keep themselves alive and free from symptoms of insufficiency. In some animals the salt intake increased fifteen fold after adrenalectomy (Richter, 1936).

What is the basis of such selective behavior? "The evidence at hand indicates," writes Richter, "that the self-regulatory dietary behavior in rats depends largely on taste and not on experience" (1942, p. 565). He has found that electrolytes are accepted by rats according to their dietary value in such low concentrations that they could not have been beneficial; that the preference threshold for saline solutions in adrenalectomized rats is much lower than in normal rats (in the ratio of about 1 to 15); and that section of all taste nerves in adrenalectomized rats prevents differentiation of saline solution from water.

What is the specific hypothesis advanced by Richter?

"It would seem . . . that nutritive deficits produce physico-chemical changes throughout the entire body, including the taste mechanisms in the mouth, and that these changes may entirely alter the taste for different substances. In response to these

changes an animal may be stimulated to seek certain substances in much the same way as dehydrated animals are stimulated by a dry throat to seek water. On the basis of theoretical considerations on appetite and hunger, Mursell (1925) arrived at very similar conclusions. Thus he states 'the best hypothesis covering the facts is that of certain positive chemotropisms which operate to set up cravings for specific substances'" (Richter, Holt, and Barelare, 1938, pp. 742-743).

Young.—Young's studies of food preference in rats were begun in 1928 and are still continuing today. They constitute by far the most extensive integrated body of data available on the subject. Young has used a number of different methods, but on the whole has concentrated upon frequency of choice as a criterion of preference rather than relative amount consumed (Richter's criterion). The outstanding feature of Young's work is that it shows that many factors are involved in the determination of food choices besides the nutritive deficiencies stressed by Richter.

An excellent demonstration that need is not the sole determinant of preference for alternative concentrations of a substance nor for intake of that substance is to be found in an as yet unpublished thesis reported by Young (1948a). The salt intake of rats, both adrenalectomized and normal, was determined when the source was a single 3 per cent solution and when it was a set of 8 solutions varying from 0.1 per cent to 12 per cent. In the latter case the rats, both adrenalectomized and normal, showed a clear preference for a solution of 0.7 per cent. Accepting Richter's evidence that the salt need for adrenalectomized rats is greater than for normal ones, preference here turns out to be independent of need. Furthermore, the intake of salt, for both categories of rats, was 1.5 to 3.5 times greater when the rats had access to the 8 alternative solutions than when they had access to only one. This shows clearly that need is not the sole determinant of intake, but that the nature of the foodstuff also plays a great part.

This latter point is also demonstrated in an as yet unpublished experiment of Young on the amount of solutions of sucrose and sodium chloride consumed by rats when need is held constant and concentration of solution is varied. The intake of the substances (as

opposed to the solutions) varies with concentration. Maximal intake of substance occurred in the case of sucrose at 18 per cent, in the case of sodium chloride at 2 per cent. The optimal concentrations, i.e., those yielding maximal intake of solutions, were at 8.5 per cent for sucrose, 0.7 per cent for sodium chloride, and represented maximal intakes of water, not of the solutes.

From these and other results Young derives the concept palatability, which plays a key role in his interpretation of nutritive behavior. "The term palatability refers to the acceptability of foodstuffs as determined by the characteristics of the food stimulus. . . . An animal accepts what he *likes* as well as what he *needs* and it is an open question as to how far what he likes agrees with what he needs" (1948a, p. 303).

Habit is another very important concept in Young's interpretation of nutritional behavior. "In all of the work upon food-seeking and food-selecting behavior it is obvious that the animal learns those instrumental acts which lead to food" (1948a, pp. 307-8). Young has shown that speed of running to food increases with practice, and also with palatability, as measured with a preference test. In connection with these findings he develops the hypothesis "that the rat when repeatedly placed in a situation which yield food, builds up a specific neuromuscular determination which is capable of regulating the pattern of behavior. There is a specific determination to run to food or to do with the laboratory gadgets whatever is necessary to obtain food. This specific determination is related to what Tolman and others have meant by expectancy" (1948a, p. 308). It derives its driving power, he believes, from proprioceptive tension. This tension varies directly with palatability.

But how does this view account for the influence of need on food-seeking behavior? Young has found it difficult to reverse established food preferences by deprivation of the less preferred food, but easy to do so by satiation with the preferred food. Because of this, he writes,

> "The hypothesis on which we are working is that the inhibition of specific food acceptance through satiation can explain the selecting and balancing of a diet in accordance with bodily needs

just as adequately as the hypothesis that every need creates its own specific hunger which drives the animal to go forth and seek the food which is needed. . . . According to the satiation theory of food selection an animal ingests a food until a need is met and perhaps more than met. Some inner mechanism (not clearly understood) puts on the brakes, inhibiting further ingestion at satiation" (1948a, p. 299).

Three of the recent studies of Young have been concerned with the relation of palatability as measured by preference tests, to *rate of running*, on the one hand, and to *rate of learning*, on the other (1946, 1947, 1948b). The experiments show clearly that rats *run faster* to a preferred food When, however, speed is equalized, there remains no evidence that they learn faster.

These findings Young interprets by distinguishing two factors in the learning situation, strength of drive and rate of habit growth. "Strength of drive, as measured by the time required to approach and accept a food, is positively correlated with the degree of palatability of the incentive. Animals run faster in approaching a highly palatable food than in approaching one of low palatability. The rate of habit growth, however, is not dependent upon the degree of palatability of the incentive. Learning depends upon the frequency and distribution of reinforcements" (1948a, pp 316-317).

But what of the law of effect? How explain the obvious fact that selective behavior develops with practice? The law does hold, Young believes, but only for motive "If learning is defined as the acquisition of patterns of behavior through practice the law of effect is not valid as a law of learning. But if the definition of learning is broadened to include the acquisition of motives (drives and specific food expectancies) as well as motivated behavior patterns, then it can be said that there are two independently variable determinants of such acquisition: (a) practice and (b) effect" (Young, 1948b, p. 300).

Theories in the Framework of the Physiological Point of View

Cannon.—Like dynamic psychologists, physiologists are in the main concerned with the common-sense concepts of emotion, the

layman's constructs corresponding to these terms. Their task is essentially an attempt to specify these constructs in physiological terms. As physiologists work primarily with animals, they have been concerned more with emotion than feeling because the so-called expressions of emotion are more evident than those of feeling.

Modern physiological views of emotion may be said to start with Cannon (1929). Obviously, he did not start from scratch, but he was the first to develop a broad and integrated physiological picture of emotion. As might be expected, his picture refers only to intense instances of the major emotions, because these alone involve marked bodily changes.

According to Cannon, the reaction system involved in the major emotions has many features in common with reflexes—appearance shortly after birth, permanence, elicitation by fairly definite stimulus conditions, and usefulness. It is this latter feature which was especially stressed by Cannon. The reaction system in emotion is, he believed, a homeostatic mechanism, in the sense that it adjusts the organism to cope with emergencies. In consequence, Cannon's conception is often referred to as the Emergency Theory of emotion.

The reaction, he believed, is mediated by subcortical centers in the hypothalamus, and involves both a skeletal component and a visceral one. It is the latter with which Cannon concerned himself particularly in his experimental work. Its essential feature, he believed, was the excitation of the sympathetic division of the autonomic nervous system. This division, acting diffusely, brought about a multitude of responses by glands and muscles resulting in adjustments—e.g., increased heart rate, increased blood pressure, increased rate of coagulation of the blood, mobilization of sugar in the blood—serviceable to the organism in time of stress.

One of these responses, secretion of adrenin by the adrenal gland, was especially stressed by Cannon. Adrenin, he pointed out, is a sympathicomimetic substance, one which, when it reaches visceral effectors through the blood stream, causes them to react as they do to nervous impulses of the sympathetic nervous system. Adrenin, then, provides a means of self-reinforcement and self-

perpetuation for the sympathetic reaction. Later experiments showed that another hormone, sympathin, had like effects.

Cannon's work on the bodily reaction in emotion, together with his knowledge of somesthetic sensory physiology, convinced him that James' theory of conscious emotion was inadequate, and led him to formulate the so-called Thalamic Theory. In a nutshell, where James' theory made conscious emotion the awareness of bodily response through sensory channels, Cannon's made it the awareness of the antecedent thalamic processes, mediated through thalamico-cortical channels. These two theories of conscious emotion have for many years constituted the mainstay of theoretical discussions of emotion in textbooks. This is, it seems to me, an unfortunate case of letting the tail wag a manx cat. As Hunt (1948) points out, such views as these assume a specific "emotional" conscious content which has not been demonstrated experimentally.

In terms of space in print, it is quite clear that Cannon's major concern was with the bodily reaction in emotion, not with conscious emotion. In the first edition of his book *Bodily Changes in Pain, Hunger, Fear and Rage* twelve chapters deal with the mechanism of the bodily reaction, one with the problem of conscious content. Roughly the same proportion is maintained in the second edition. It is this extensive and highly integrated body of knowledge of the bodily reaction in emotion which has constituted the jumping-off point for many of the recent developments of the theory of emotion in the framework of physiology.

Role of Autonomic System in Emotion. These developments have centered around four major problems. One concerns the role of the autonomic system in emotion. As pointed out, emotion, for Cannon, was essentially (although not exclusively) a sympathetic reaction. Recent work indicates that there is also a considerable involvement of the parasympathetic division of the autonomic system. Thus Gellhorn has shown the participation of what he terms the vago-insulin system in emotions, including sham rage so extensively studied by Cannon (Gellhorn, 1943, pp. 195-200). Cannon stressed tremendously the role of adrenin in the emergency function of the organism. Rogoff (1945) has presented contrary evidence

Arnold (1945) has reviewed this problem recently and proposed a view of the autonomic reaction in emotion which relates various emotions to specific combinations of sympathetic and parasympathetic excitation.

Role of Thalamic Centers in Emotion. A second problem emphasized in the recent development of the theory of emotion in the framework of physiology concerns the role of the thalamic centers. Cannon had stressed this role largely because of the earlier work of Head on patients with brain lesions (Head and Holmes, 1911). Head had interpreted his data to show that the thalamus was the center concerned with the affective factor in sensation. Lashley has re-examined Head's findings and has pointed out that

> "The supposed evidence that the thalamus adds the affective or emotional character to sensations breaks down completely when subjected to critical analysis. . . . There is no evidence whatever that the thalamus contributes facilitative impulses which might form a basis for the motivational aspects of emotion. Thus the only part of the thalamic theory of emotion which has factual support is the localization of motor centers for emotional expression within the hypothalamus. It seems certain that these motor centers do not contribute directly to other aspects of emotion and there is no evidence for the existence of other affective or emotional centers" (1938, p. 60).

Essentially the same conclusion is reached by Masserman as a result of extensive experiments on the subject, including the direct electrical stimulation of the hypothalamus with implanted electrodes in fully recovered and quite normal cats. The results with this latter procedure are so crucial to the problem under discussion it seems well to quote some of them before stating Masserman's conclusion.

> "When a 60-cycle current of from 2 to 4 volts is applied through an implanted bipolar needle electrode directly to the hypothalamus of an unanesthetized, freely moving cat, the animal begins to retract its ears, crouch, growl, raise its back, and lash its tail, and show a crescendo of the following typical sympathetic and motor reactions: hyperpnea, salivation, mydriasis with widen-

ing palpebrae, piloerection, biting and striking movements with claws unsheathed, and, finally precipitate running as though in blind attempts to escape . Nevertheless, much as these reactions resemble those of rage and fear, they differ from the latter in certain significant respects For instance, the ostensibly aggressive activity during hypothalamic stimulation is not directed toward specific objects in the animal's environment, even when these are manipulated so as directly to irritate the animal Again, the responses induced by hypothalamic stimulation are not adapted to the surroundings, e g , the cat will dash itself repeatedly against the side of the cage and neglect a readily available avenue of escape Moreover all the pseudo-affective reactions cease abruptly at the end of the stimulus without leaving any of the residua (mewing trembling, hiding, etc) ordinarily observed after true emotional states

"Within 36 hours after the aseptic implantation of electrodes into the hypothalamus a properly operated cat in most instances will show normal responses to petting, feeding, restraint, threats by dogs, and other emotionally meaningful external situations If the hypothalamus of the animal is then stimulated electrically while the animal displays such normal reactions its behavior will continue almost unchanged until mechanically interfered with by the motor components of the pseudo affective responses For instance, the animal will often continue to lap milk, purr, clean its fur or respond to petting during hypothalamic stimulation, despite the appearance of mydriasis salivation, horripilation, and other typical manifestations of 'sham rage' When these observations are contrasted with the immediate abandonment of feeding, purring or contrectative activities under the influence of true fear or rage produced for instance, by the sight of a dog, the conclusion becomes evident that faradic stimulation of the hypothalamus produces either no significant change in emotional attitude or that the affective modifications are so minor as to exert no significant influence on normal behavior patterns" (1943, pp 35-37)

From these and other data Massermann concludes

"It would seem safest, in the present state of our knowledge to assign to the hypothalamus its experimentally demonstrable role in reinforcing and co ordinating the neural and hormonal mechanisms of conative and emotional *expression* and reserve for more adequate proof the hypothesis that it is either

the dynamic source or the seat of experience of affective states. On the other hand, much experimental, psychological, and clinical evidence clearly indicates that an emotion is a highly integrated conative, cognitive, and affectivesomatic reaction, in which not only the central nervous system but the entire organism functions as a psychobiologic whole in its sensitive adaptations to the continually changing organismal milieu" (1943, p. 57).

Role of Hypothalamus in Emotional Expression.—The third major problem which has developed from the basic work of Cannon is that of the role of the hypothalamus in emotional expression. We have already quoted Morgan to the effect that Bard believes that anger, fear, and female estrous behavior are definite distinguishable patterns of response in the cat. Bard has studied extensively the neural control of these responses, particularly of rage

Rage in acute decorticate preparations, he found, depends upon the caudal portion of the hypothalamus "As regards exhibitions of fear, they have been seen (Bard and Rioch) in totally decorticate cats; they occur only in response to auditory stimuli The exact region of the brain stem concerned in the elaboration of this behavior has not yet been determined" (Bard, 1942, p 171). With respect to the estrous behavior, Bard states that "crouching and treading are evokable in female cats in which all parts of the brain above the mesencephalon have been removed or disconnected" (Bard, 1942, p. 172).

The picture, however, is complicated by another finding of Magoun and Bard, namely that "cats in which the entire hypothalamus was destroyed, but in which all other parts of the brain remained intact, are capable of displaying rage behavior" (Bard, 1942, p. 171).

Role of Frontal Lobes.—A fourth major problem which has its roots in the basic point of view of Cannon, but which has developed among experimental and clinical neurologists, is that of the role of the frontal lobes in emotion.

In 1936 Jacobsen published a study of the effects of removal of the frontal areas on the organization of behavior in monkeys. One of his findings concerned emotional reactions. "The animal with

out its frontal areas no longer appears to 'worry' over mistakes. Whereas the normal monkey or chimpanzee may become excited, cry and have a temper tantrum, or on the other hand, turn away and ignore the problem after several successive failures, the subject lacking frontal areas seems quite impervious to any frustrating effects of errors" (Jacobsen, 1936, p. 52).

According to Freeman and Watts (1942) Jacobsen's work, presented in a symposium on the frontal lobes at the second International Neurological Congress during the summer of 1935 in London, was one of the factors which led Egas Moniz, a Portuguese neurologist, to attempt treatment of psychotic patients by surgical destruction of pre-frontal white matter. The first operation was performed on November 12, 1935, by Moniz and Almeida Lima. Moniz found this type of operation to be particularly successful with a group of depressed patients.

Lobotomy—for such is the name used in America for this operation—has been done extensively by Freeman and Watts. The effect on the emotional life, they find, is very marked. "The mood of these patients is generally cheerful, particularly in comparison with what it had been before operation. . . . Lack of tact, childishness, wisecracking, *Witzelsucht*, playfulness, and singing are occasionally observed, while seriously disturbing aggressive behavior, indecency, profanity and other disturbing features are fortunately quite exceptional. . . . As the patient recovers from the effects of the operation, there is a gradual increase in the exercise of restraint over his annoying activities. . . . The freedom from painful self-consciousness, and also from preoccupations with former conflicts, repressions, frustrations, and the like, and the associated elevation in mood, renders life particularly agreeable to them, and they enjoy it to the full" (Freeman and Watts, 1942, pp. 208-9).

Conclusion. What has been the effect of the work with respect to these four problems upon the theory of emotion within the framework of physiology? Essentially, a marked expansion and complication of the picture painted by Cannon. Summarizing research in this field, Morgan rejects Cannon's notion that emotion is "localized" in the diencephalon. Instead, he writes,

"Fundamental sympathetic responses depend only on the spinal cord and medulla. Certain items of somatic behavior are mediated by the medulla and midbrain. These are more perfectly integrated in the hypothalamus, but certain additional components of emotional behavior are mediated by the thalamus and striatum. The most complete expressions, involving first the restraint of emotion and later its maintenance, including localizing behavior with respect to the emotional stimulus, depend upon the intact cerebral cortex. Thus, in emotion, as we have seen elsewhere in sensory and motor systems, different sorts of integration are performed at different levels of the nervous system, and no one nucleus or region can be said to constitute the neural center of the emotional behavior" (Morgan, 1943, p. 373).

A consequence of this expansion and complication of the physiological picture of emotion has been to bring the picture more in line with views stressing the motivational character of emotion—dynamic views, and those of the more recent behaviorists. Thus Morgan points out that the c.m.s. (central motive state) which he equates to motivation has the same general properties in emotion as in hunger. In particular, the properties of self-perpetuation and of "priming," i.e., of constituting a set for action which satisfies the motive, are present in both cases. The difference is one of conditions of elicitation. An h.m.f. (humoral motive factor) is involved in the case of hunger, an external stimulus in the case of emotion. This view is obviously close to that of Skinner.

Development of physiological work on emotion has had another consequence. The evidence leading to the expansion and complication of the neural picture has been largely negative. Involvement of the cortex is inferred in the main from the incompleteness of emotional reactions in thalamic animals or in normal animals subjected to thalamic stimulation. The result is that much of the expanded neural picture is extremely indefinite. In the heyday of Cannon replacement of psychological constructs in the theory of emotion by physiological ones seemed just around the corner. Today such replacement seems more distant—indeed, perhaps even somewhat problematical.

Conclusions

Development of the Theory of Feeling and Emotion.—The principal development in the theory of feeling and emotion during the last quarter of a century appears to have been the substitution of construction for reification. The layman takes it for granted that if there is a word, there is a corresponding thing. The statements "I am angry" or "that person is angry" are for him indices of a specific state in the organism referred to. This was essentially the view of psychologists, too, in the early years of the present century. Their task, it seemed, in the field of feeling and emotion, was simply to study the set of states referred to by such words as anger, or fear, or pleasure. A Titchener sought to do this by introspection, a Freud by psychoanalysis, a Watson by observing behavior.

Research in all three of the fields has led to the abandonment of this view. Introspective psychology failed to find any specific conscious content corresponding to these forms. Dynamic psychology was obliged to assume "unconscious" conscious states to develop its system of motivation. Behavioristic psychology failed to identify, at least in man, specific reactions to use as behavioral equivalents of the terms.

Meanwhile, psychology in general was becoming construct-minded. Experimental evidence forced Titchener, in 1915, to make sensation a construct (Boring, 1942). To keep behaviorism distinct from physiology and to allow it to deal with the classical problems of psychology, Tolman, in 1922, proposed the use of what he later would have called intervening variables. In 1932 he made such intervening variables, now called by this name, the basis of his psychological system. In 1935 Stevens pointed to the need, in psychology, of the operationism already applied to physics by Bridgman (1927), and his argument received widespread acceptance (1935a). For operationism, as Stevens has put it, "there are *only* constructs, first, last and always. . . A datum *is* a construct. Those who distinguish between them appear to do so on the basis of simplicity: the more elementary discriminations are called data, and the more complex ones are called constructs. There can, of course, be no objection to

a distinction between simple constructs (of low order) and complex ones (of high order)" (1935b, p. 523).

As far as the theory of feeling and emotion is concerned, this view of the nature of scientific concepts was tremendously important. It made it possible, in the first place, to evaluate properly the layman's concepts of feeling and emotion, and the closely related ones of the psychoanalytic school These concepts, it seemed clear, were intuitive constructs, reifications derived from a variable set of unspecified operations. As such they had the worth of being based upon a tremendous factual background, but the weakness of being extremely vague They could outline an important systematic universe, they could be used with considerable success in the development of common-sense systems of motivation, but their status relative to the operational variables of modern experimentation was woefully indefinite.

The view that scientific concepts are constructs made it possible, in the second place, to start developing alternative theories of feeling and emotion from the bottom up So long as feeling and emotion were considered to be basic "givens," no competition was possible There could be but one psychology of feeling and emotion, and that was the introspective one. The behaviorists, forced to give up this old view, were also forced to develop a new one. Unable to take over feeling and emotion as givens, they proceeded to construct them, or alternates for them, from their behavioral data. The result was the modern theories as they are represented in the major systems of Tolman and Hull and Skinner, and in the more restricted systems of Hebb and Young. It will be a long time, naturally, before these earthy theories develop the scope of the older ethereal ones, but they will constitute an integral part of modern psychology, not a substandard universe dealt with in textbooks out of respect for tradition.

Detailed Exemplification of a Modern Construct: Palatability.—In what direction do these earthy modern theories point? Let us try to answer this question for feeling, and, to avoid vagueness, for a restricted factual universe, namely that of nutritive behavior in rats. In this universe, feeling seems at present best represented

by Young's concept of palatability, "the acceptability of foodstuffs as determined by the characteristics of the food stimulus" (Young, 1948a, p. 310).

The evidence is clear, as we have seen, that the nutritive behavior of rats can be manipulated by changes in the available foodstuff, in

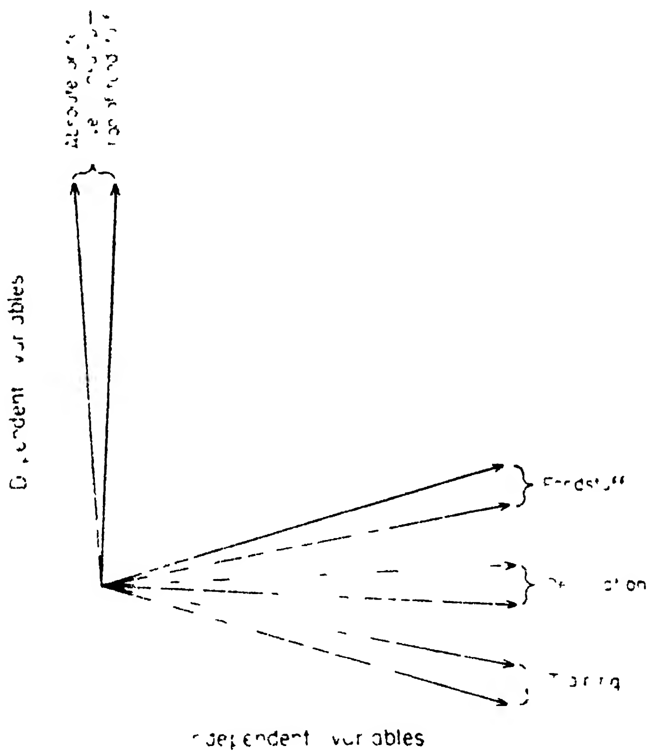

FIG. 6.3 Schematic drawing showing the principal classes of variables involved in studies of food selection and consumption

deprivation, and in training. We thus have a situation where a class of dependent variables (measures of nutritive behavior) is a function of three classes of independent variables (changes in the foodstuff, in deprivation and in training). This situation is represented schematically in Figure 6.3.

We could refrain from developing any constructs, in this situation, except the experimental variables mentioned above. In this case, however, the multiplicity of relations would be tremendous.

Consider how numerous would be the variables in the case of deprivation alone. Young quotes Loosli as listing nutritive requirements of laboratory animals under 24 different headings (1948, p 290).

We note, however, that the relation between the class of dependent variables and any one class of independent variables, with the two other classes of independent variables held constant, is such that the different independent variables are to some degree alternates for the production of a given variation in a dependent variable; and, further, that the different dependent variables are to some degree

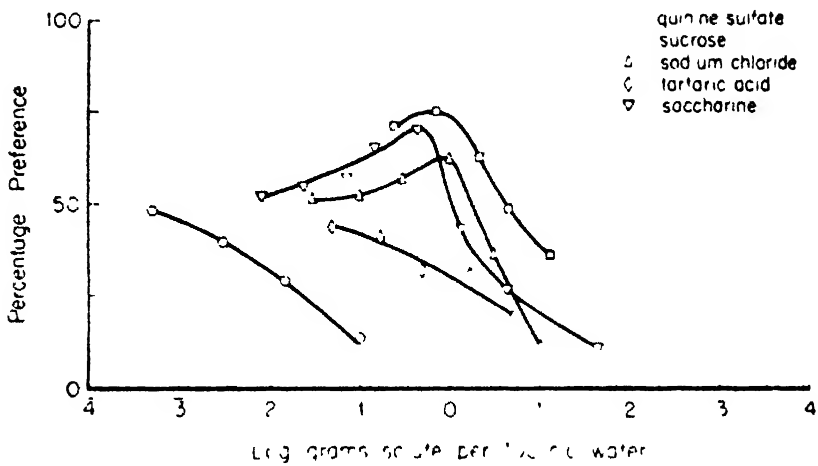

Fig. 6.4 Relation between percentage preference for a solution and its concentration, for five different types of solutions. Method of measurement held constant (bottles containing solution and water presented on alternate days.)

alternates with respect to registering the effect of variation of any one independent variable

To represent this circumstance, we assume an intervening variable, the properties of which are that it is related to the dependent variables by a set of response equivalences and to the independent ones by a set of instigation equivalences. Depending on which class of independent variables is being varied experimentally, while the two others are held constant, the resultant construct will be palatability, drive, or habit.

It is the first of these constructs, palatability, in which we are interested. Figure 6.4 represents the effect on rats of varying the

concentration of five different solutions (i.e., the effect of five independent variables) on one dependent variable, according to data of Beebe-Center, Black, Hoffman and Wade (1948) and to unpublished data of Beebe-Center and Wade. The dependent variable is per cent preference defined as 100 S/(S + W) in which S and W represent *per diem* consumption, respectively, of a sapid solution and of water presented on alternate days. Figure 6.5 represents the effect of varying a single independent variable, concentration of a

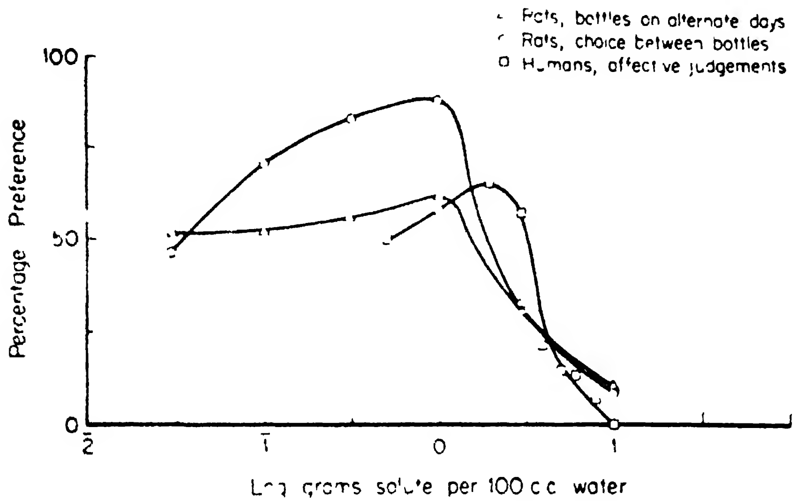

Fig. 6.5. Relation between percentage preference for a solute and its concentration for three different methods of measuring preference. Type of solution held constant (sodium chloride).

solution of sodium chloride on three different dependent variables. One is percentage preference for human beings defined as in Figure 6.2. Another is percentage preference for rats defined as in Figure 6.4. The third is based on unpublished work of McLeod and Liss (McLeod, 1947). It is percentage preference defined as in Figure 6.4, except that the saline solution and the water were not presented separately on alternate days, but were both available to the rats at the same time.

Data such as these make possible a strict definition of palatability, but only by adopting a specific set of conventions. By far the sim-

plest such set is that some one dependent variable is *the* measure of palatability and that the others are related to palatability as they are to it. We could, for instance, consider the dependent variable of Figure 6.4 as such a criterion variable In that case the figure would show directly the relation of palatability to the concentration of five solutions, and Figure 6.5 would provide the basis for plotting the relation of three dependent variables to palatability; one, the criterion variable, being a linear function of palatability This pro-

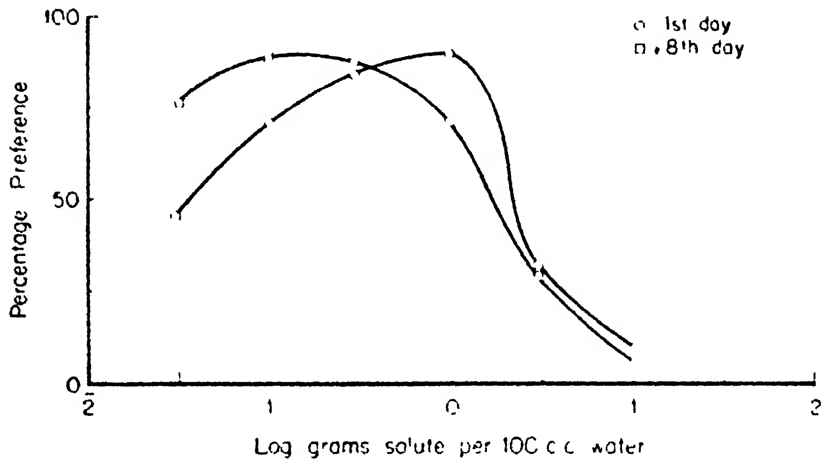

FIG. 6.6. Relation between percentage preference for a solution and its concentration for two different degrees of training. Method of measurement (solution and water available simultaneously) and type of solution (sodium chloride) held constant

cedure, however, would be wholly arbitrary. Not until we have many more data on the order of those shown in Figures 6.4 and 6.5 will adequate reasons develop for choice of one set of conventions rather than another.

Meantime, what are we to do? Exactly what experimenters have usually done under similar circumstances in other fields: consider the dependent variables as *indices* of palatability, whose exact relation to palatability is to be determined in the future. In so doing, however, the fact of so doing should ever be kept in mind with its implication that little can be expected of such an index except that it will be a monotonic increasing function of palatability as ultimately defined.

All of the discussion of palatability above has assumed that the independent variables representing deprivation and training are held at some one constant value. From what was said at the beginning of the discussion it is clear that for some other sufficiently different constant value of these parameters the functions relating palatability to the variables specifying the foodstuffs are going to be different If the interrelation of the various dependent variables remains the same for different values of the parameters, so that the same set of conventions can be used relating the dependent variables to palata-

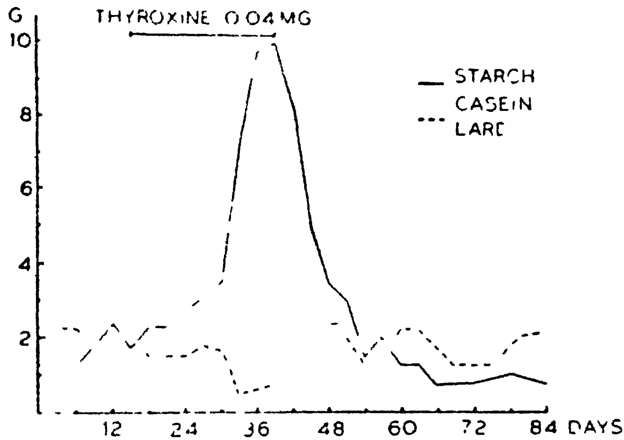

FIG 6.7 Effect of administration of thyroxine on consumption of three different foodstuffs. (From S Donhoffer and J Vonotzky, *Amer J Physiol*, 1947, 150, 334-339, by permission of the authors and The American Physiological Society)

bility, the effect of these parameters can be measured by determining their effect on any one dependent variable.

Figure 6.6 from McLeod shows the effect of 7 days of training on percentage preference for salt solutions, as defined above in dealing with McLeod's data If we accept this dependent variable as an index of palatability, this figure shows the relation of palatability to training in a special case It should be noticed that on this assumption training here increases palatability for concentrations below about 1.6 log gram solute, and decreases it for higher concentrations This circumstance indicates well t. fact that variation of the parameter training does not have a uniform effect on palatability, but one which varies from foodstuff to foodstuff.

This selectivity of effect also occurs in the case of the parameter deprivation, as was shown by Richter, Holt and Barelare (1938). Figure 6.7, from Donhofer and Vonotzky (1947), is a striking illustration of this effect provided administration of thyroxine be considered a form of deprivation.

Palatability as a Prototype of Affective Constructs.--We have tried to sketch a fairly detailed derivation of Young's construct palatability, which represents feeling in the restricted universe of nutritive behavior. Surely, this is a small part of the total experimental universe of feeling and emotion. Why this one-sided emphasis?

There are two reasons. The first is that the universe of nutritive behavior is the only one to provide adequate material for the detailed definition of an affective concept. Only in this universe is it possible, at present, to illustrate concretely and step by step the generation of such a concept from actual experimental data

The second reason is that palatability may be considered a prototype for the development of other concepts in the fields of both feeling and emotion. That this is the case for feeling is obvious Except for attempts to have observers specify affective conscious elements, the experimental psychology of feeling has been essentially a correlation of measures of preference, as dependent variables, and of measures of stimulating conditions, as independent ones, with occasional attempts to vary the parameters deprivation and training. The whole experimental universe of feeling thus corresponds closely to the restricted universe underlying palatability.

It is also the case for emotion. The basic fact of the psychology of both feeling and emotion is that certain stimulus conditions are prepotent, that they stand out from all others with respect to magnitude of effect on conduct This common factual basis is strong presumptive evidence that a type of concept appropriate for one field will also be appropriate for the other. The best demonstration, however, that palatability may be considered a prototype of concepts in the field of emotion is that our procedure, in deriving the concept palatability, is simply an adaptation of Skinner's procedure in defining emotion.

Let us return, now, to the question asked at the beginning of the preceding section, the question, namely, as to the direction in which modern "earthy" theories of feeling and emotion point. The bearing, on this question, of the discussion above is quite obvious. The only such direction which we can at present define by detailed exemplification is that represented by the use of palatability as a prototype.

This is not to deny, naturally, the possibility of other answers. To begin with, we are not obliged to construct affective concepts at all. Hull's Behavior Theory, which purports to cover the whole field of behavior, does not, as we have pointed out, include any strictly affective concepts at all. Furthermore, affective concepts derived according to different procedures may ultimately turn out to be more useful than concepts modeled after palatability. To discuss the theory of feeling and emotion profitably, however, we need some concrete exemplification of affective concepts, and palatability provides at present the best such prototype.

Is this to say that the task of theory in this field is purely a matter of generating concepts on the order of palatability? Certainly not. In a sense the task is less than this, in a sense it is more. It is less because, except for practical reasons such as those which happen to exist in the case of palatability, no one is going to devote to any single affective concept the time necessary to provide a complete specification of it. It is more, because theory is not concerned with concepts such as palatability for their own sakes but only for the light they throw upon the structure of affective concepts in general. Acceptance of palatability as a prototype thus does not provide a model for the mass production of affective concepts, but a schema in terms of which we can analyze affective behavior in order to get at its basic principles and mechanisms.

Modern Experimental Issues. We shall now consider what the principal experimental problems would be in the field of feeling and emotion, if palatability, as defined above, were to be considered the prototype of constructs in this field. In doing so, we shall indicate, for each type of problem, some of the more recent experimental work which bears upon it. This procedure will keep the

presentation concrete. It will bring out a number of minor problems which are relatively independent of the general point of view held by the investigator. Most important, it will offset the bias involved in defining the trend of modern theory by exemplification, as we have done above. It will provide the reader with material from which he can judge for himself the direction in which the theoretical views of experimentalists are at present pointing in this field.

Dependence on Eliciting Conditions.—The study of the relation of feeling and emotion to eliciting conditions has had very different degrees of success. By and large, it has yielded stable data in the case of feeling, unstable ones in the case of emotion.

The problem is being very intensively studied, at present, in the case of palatability in rats. Here the issue is the role of ***sensory mechanisms.*** Palatability, for Young, is directly related to sensory stimulation. He writes: "The term palatability refers to the immediate affective reaction (liking or disliking) of an organism which occurs when a food stimulus comes in contact with the head receptors" (1948a, p. 310). Although Richter does not use the concept of palatability, he likewise relates acceptability to sensory mechanisms—as we have seen, he believes that dietary selection in rats depends principally on taste. A good deal more work needs to be done on this relationship, however, before we know it in detail—and, indeed, before we are sure that it exists.

Pfaffmann and Bare have found that rats show preference for saline solutions after "combined lingual (including chorda tympani) and glosso-pharyngeal (IXth) nerve removal" (1948, p. 254). Many years ago Gamble, Putnam and McKhaim (1929) showed that increase of salt in the food of rats determines increased water intake. Their data can account well for the positive phase of available ("preference") curves for saline solutions without recourse to stimulation of salt receptors. Sparer (1948) has shown that saccharine in the food of rats affects hardly at all their *per diem* food intake, but increases their water consumption. His data account well—again without recourse to taste receptors—for the positive phase of the "preference" curve of rats for saccharine solutions published by Beebe-Center, Black, Hoffman and Wade (1948).

These data do not yet disprove the views of Richter and Young concerning the role of head receptors in food selection. In particular, salt or saccharine ingested in food may make the drinking water taste salty or sweet to the rats. There is no question, however, but that they lend support to Adolph's extreme view that food acceptance has little to do with the organs of taste (1947).

An interesting attempt has recently been made by Hsu (1946) to specify by multiple *factor analysis* the dimensions of olfactory stimulation which are related to feeling. This technique might yield extremely interesting results, when applied to foodstuffs, if one were to use the variable "appetite level" developed by Dove (1943). Incidentally, Dove's work shows a remarkable divergence between value of foods in terms of "appetite level" and in terms of nutritive value.

Another issue the solution of which may be greatly facilitated by modern techniques is that of the *relative effectiveness of different modalities* in eliciting feeling. Ruch writes: "Certain modalities of sensation are strongly pleasant or unpleasant, e.g., pain, while others are relatively neutral, e.g., muscle sense" (1947, p. 306). Accurate specification, here, requires development of a common scale of sensory intensity—possibly by extending beyond a single modality the matching procedure used by Beebe-Center and Waddell (1948)—and choice of an index of feeling with very general applicability—possibly percentage preference as used in Figure 6.4 or affective power as defined by Beebe-Center and Beebe-Center (1946).

In regard to the relation of emotion to eliciting conditions modern research has followed two principal directions. One is concerned with *objects as eliciting conditions*. A good example is the work of Haslerud (1938) on the fear-inducing value of movements of various objects in the case of chimpanzees. Another is Hebb's (1946b) study of the reactions of chimpanzees to various objects "representing" primates (e.g., the head of a chimpanzee, the skull of a chimpanzee with movable jaw, etc.). Another direction is concerned with *complex situations*, e.g., conflict, as *eliciting conditions*. Good examples, here, are Dembo's study (1931) of the arousal of emotional tension by frustration, and French's study (1944) of the

frustration in a social situation. A complicating factor in such studies is the prominence of individual differences, which has led Rosenzweig (1944) to develop the concept of frustration tolerance.

Relation to Responses.—Feeling and emotion are truly all pervasive. Almost any dependent variable taken as an index of feeling or emotion will be found to be correlated in some degree with a tremendous number of other dependent variables. Classic instances are provided by studies of the skeletal and visceral expressions of emotion (Young, 1943). The field of such interrelated dependent variables has recently been extended to include perceptual discriminations by Bruner and Postman (1947). It is this multiplicity of correlated responses which—together with the multiplicity of independent variables descriptive of eliciting conditions—has led to the modern views of feeling (cf. Young) and emotion (cf. Skinner) as intervening variables.

A basic issue, here, concerns the primacy of a restricted group of these responses. Skinner does not stress the role of any particular response system in emotion. No one set of responses acts as a mediator for the other. Miller, on the other hand, interprets the effect of external emotional stimuli on conduct as largely a two-step process, the arousal of certain specific responses which give rise to strong response-produced stimuli (the emotions proper), and the subsequent responses to these response-produced stimuli.

A special case of the issue is particularly important. It concerns the role of hypothalamic and autonomic processes in emotion. Are these processes, about which we now know so much, the core of emotion? This would accord with Miller's view. Or are they simply one manifestation, coordinate with any others, of a general change in reflex strength? This would accord with the position of Skinner.

Highly germane to this issue are modern studies of facilitation and inhibition in the nervous system such as those of Murphy and Gellhorn (1945a and b), Bach and Magoun (1947), Lloyd (1946), Larrabee and Bronk (1947), and Marrazzi and Marrazzi (1947). A bridge here between the work of the psychologists and that of the physiologists would constitute a tremendous advance in the theory of emotion.

Dependence on Learning.- In the case of feeling, a first problem in this field has been the practical one of finding an index which is minimally sensitive to training within the experimental situation An attempt in this direction has recently been made with respect to nutritive behavior in the rat by Beebe-Center, Black, Hoffman and Wade (1948). Their index shows reasonable independence from training, but is subject to vitiation by uncontrolled deprivation factors, as was pointed out in discussing the relation of palatability to eliciting conditions The difficulty of the problem is brought

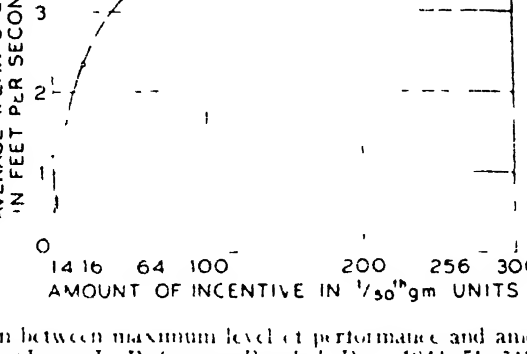

FIG. 6.8 Relation between maximum level of performance and amount of incentive used in training (From L P Crespi, *Psychol Rev* 1944 51 341-457, by permission of the author and the American Psychological Association)

home when one considers that introspective indices are markedly influenced by serial effect (so-called law of affective equilibrium) (Beebe-Center, 1932)

An important source of information on the relation of palatability to learning is some of the modern work on reinforcement. Hull (1943), on the basis of available evidence, concludes that the asymptote of learning curves increases with increase in the amount of reinforcement, rate of approach to this limit remaining constant Figure 6.8, from Crespi (1944), demonstrates this conclusion (level of performance, here, represents asymptote of learning curve). Hull further suggests that a like relation would hold for changes in quality of the reinforcing agent. Figure 6.7, a few pages back, supports this suggestion, but more data are obviously needed here.

These findings go far toward laying to rest a ghost which has recently haunted the psychology of feeling. Constancy of training, for different eliciting conditions, may be achieved within the experimental situation, but is difficult to guarantee for the pre-experimental life of the animal. In consequence, once the dependence of indices of palatability on learning had been recognized, it always seemed possible that the palatability of a substance might represent nothing more than degree of training with respect to that substance. Data like those of Peters (1939), showing that repeated pronunciation of strange words increases their affective value, or those of Duncker (1938) or of Marinho (1942), showing that selection of food induced by prestige factors outlasts the operation of these factors, would support this possibility It becomes very remote, however, in view of the difference in asymptotes of learning curves for different reward conditions. In the case of palatability, at least, feeling is certainly related to the characteristics of the foodstuffs

Is this relationship simply based on reinforcement value? This possibility was implicitly suggested earlier, when it was pointed out that the modern law of effect grew out of the older doctrine of hedonism of the past. Young, we have seen, found speed to vary with palatability, but not learning when the speed factor was equalized (Young, 1948a). This finding tends strongly to divorce palatability from reinforcement in the sense in which it is usually used, in the sense of reinforcement of behavior patterns As Young points out, more work is needed on this problem.

Let us now turn to emotion. If we interpret the problem here in a broad sense, the literature is tremendous, for it includes the writings of the whole psychoanalytic school. A striking contribution in the clinical field is the work of Grinker and Spiegel on battle fatigue (1945). Outstanding contributions in the experimental literature are those of Watson (1919) on conditioning of emotion in children, of Liddell (1944) and his associates and of Masserman (1943) on experimental neuroses, and the work described above, by Skinner, Estes, Mowrer and Miller, on the development of anxiety.

The key issue in this field is whether the dependence of emotion on learning is a direct one, or one mediated through cognitive

learning. The former point of view is most clearly represented by Watson. The latter is stated very clearly by MacCurdy. In dealing with anxiety states he writes: "The anxiety is directed against mental images of a dangerous nature which in the psychoses come into full consciousness. The fear, as such, is rational, if the reality of the stimulus were only granted" (MacCurdy, 1925).

Dependence on Deprivation.—The general relation to deprivation has already been described for palatability in connection with the formal development of this construct. Richter's data (1936) show that salt deprivation increases the palatability of saline solutions over a wide range of concentrations. The data of Richter, Holt and Barelare (1938) and also those of Donhofer and Vonotzky (1947) show that deprivation with respect to one substance will alter in a complex way the palatability of other substances (cf. Figure 6.7). There is great need of further data in this field.

Mayer-Gross and Walker (1946) have reported a striking case of dependence of sucrose preference on blood-sugar level. They presented to their subjects 5 samples of liquids—5 and 30 per cent solutions of sucrose, a saccharine solution of the sweetness level of the 30 per cent sucrose solution, a 0.5 per cent saline solution, and plain water—with instructions to choose the one they would prefer for a long drink. The blood-sugar level of the subjects, schizophrenics being subjected to Sakel's insulin therapy, varied from a fasting level to incipient hypoglycaemic coma. Figure 6.9 shows the percentage of subjects, from groups whose blood-sugar level fell within successive 5 mg./100 ml. ranges of blood-sugar level, who preferred the 30 per cent sucrose solution to all others.

The main theoretical issue concerning the relation of feeling to deprivation arises from the work of Young. He found, it will be remembered, that established feeding habits can be shifted more readily by satiation than by deprivation. This led him to the hypothesis that selection of diet according to bodily needs depends, not on the development of specific hungers, but rather on the development of specific satiations. Should this view prove correct, it would allow the integration of data such as those described above with the extensive data on "psychological" satiation gathered by Lewin and his collaborators (1946).

As far as emotion is concerned, the most relevant body of experimental information is the literature on frustration and conflict (Dollard, Doob, Miller, Mowrer and Sears, 1939; Miller, 1944; Sears, 1944; Rosenzweig, 1944). It is clear that in certain cases emotion varies directly with deprivation (Sears and Sears, 1940). In others, however, no such relation seems to hold (Seward, 1945). A difficulty is that this literature is concerned with emotion only as one of many possible manifestations of frustration. What is needed here is experimentation directed specifically at the relation of emotion to deprivation.

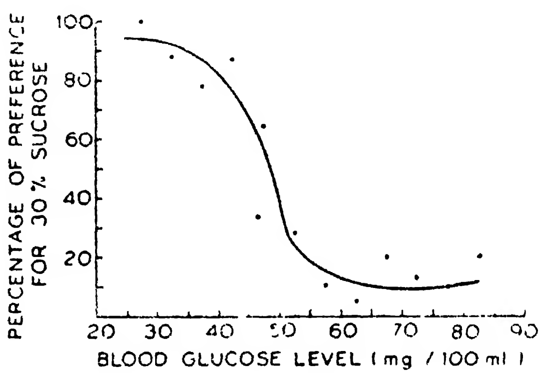

FIG. 6.9. Relation between preference for a 30% solution of sucrose, as compared to liquids of lesser sweetness, and blood glucose level. (From W. Mayer-Gross and J. W. Walker, *Brit. J. exp. Path.*, 1946, 27, 297-305, by permission of the authors and the Editor of the British Journal of Experimental Pathology.)

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CHAPTER 7

FATIGUE AND EFFICIENCY

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The purpose of this chapter is to consider the thinking that underlies studies of fatigue and efficiency. Failure to consider the assumptions upon which experimentation is premised makes such a procedure as is here proposed of especial value It seems that studies of fatigue and efficiency are examined most often for their findings rather than for the reasoning involved in their planning and in their interpretation.

The plan of the present chapter is to consider, one by one, the assumptions that appear to have constituted the foundation of varied fatigue investigations during the past few decades In doing this, the broadest concepts are dealt with first From these we pass to concepts of more restricted implication, remembering that not all investigators have been aware of all of the assumptions that are stated here. Many of them have seldom been made explicit in the literature on fatigue. Certain workers, in accepting the experimental conclusions of others, have unwittingly accepted some of these assumptions themselves. Accordingly, these investigators might now be prone to disavow some of the assumptions discussed in this chapter. It is nonetheless appropriate to examine the logical structure underlying the fields under discussion even though much of the framework has not been made explicit.

The most general assumption that underlies the modern study of biological activity is that *activity modifies the organism that acts.* Although this assumption did not arise long ago as an *a priori* proposition full-fledged and intended for guiding inquiry, we can look back now and see that it is the logical basis for much that has been done in the fields of *fatigue* and *learning.* Were we to deal

with the idea historically, undoubtedly it could be shown to have been tacitly involved in some form or another even in the very earliest experimental studies Only more recently has it come into the clear

The organism's modification of which we speak has grown more tangible from two general kinds of evidence Various studies show that, on the one hand, there is a tendency in activity toward variety (the avoidance of repetition) and on the other there is a tendency toward slowing down of activity as it is prolonged The first tendency was noted many years ago by Hunter (1914 and 1920) in certain experiments in animal maze learning His animals behaved in such ways with regard to entering or avoiding blind alleys that he interpreted the behavior as an avoidance of repetition This tendency has also been noted in many other experiments Thorndike (1927), for example required subjects to respond to spoken words with digits from 0 to 9 The subjects could choose in each trial the digits they pleased Obviously by mere chance a given digit would tend to be given twice in succession 1 time out of 10 The actual experiments showed the percentage of such occurrences was much less than 10 per cent This was taken to mean that repetition was being avoided Whether this avoidance was unwitting or of a deliberate nature was however the subject of controversy

It is scarcely necessary at this point to present the second kind of evidence namely that activity especially when it is prolonged tends to slow down

The concepts of fatigue and impairment apply to almost every case in which performance modification is in the direction of diminution or deterioration The concept of efficiency is also involved here It pertains to the relation between performance and the cost to the performing organism In dealing adequately with the concepts of fatigue and efficiency their relation to findings in learning cannot be entirely avoided Learning is a term pertaining to changes in activity toward mastery of unsuccessfully performed tasks In fatigue the *initial* performance is the standard in learning, some sort of behavior *not yet perfected* is the standard to which the character of the behavior in question is referred

The second assumption pertains to the underlying character of the *modification involved as activity progresses. It is taken to be essentially an affair of body chemistry.* In learning it is primarily the nervous system to which the modification is referred In fatigue it is mainly the muscular system within which the change is thought to take place. In both cases the processes involved are, at bottom, thought of as chemical.

Both in learning and in fatigue, studies have not been confined to intact (integrated) organisms, but have included the investigation of isolated tissue. This is to say, that the study of learning and fatigue have not been confined to psychology, but have involved various phases of physiology, such as biochemistry and pharmacology. In the field of fatigue this has led men away from fatigue defined as the peculiar and relatively easily identified experience with which every one of us is familiar This is allowable, but the attempt to discover some connection between hypothetical conditions within tissues and the fatigue experience has led to many difficulties and far-fetched digressions from which investigators have not yet extricated themselves.

Thirdly, it can be said that *fatigue is customarily treated as an energistic affair.* This, of course, arises necessarily from the assumption that it is a tissue condition, a matter of body chemistry. Metabolic pheuomena are governed by energistic [1] laws.

Two lines of experimentation have stemmed from this assumption. One of these includes all the studies in which *muscle* activity dominates. The other includes those in which the measurement of the energy involved in *mental* activity has been attempted In the first group of studies treadmills, ergographs, and other devices for measuring exertion are employed. Some of these studies involve the use of drugs, and others, oxygen deprivation

The studies of muscle activity have become so many as to be commonplace and to be almost synonymous in the minds of many with the whole field of fatigue investigation. Some of these studies will be mentioned later as examples. This, however, is the place

[1] Energistic is a term applied to anything which deals with, expresses, or implies energy in the sense used in physics (thermodynamics). Energistic laws are those which pertain to energy transformation (food into heat, motion, etc.).

to provide some examples of studies in the second direction—the attempt to determine the cost of thinking in terms of energy expended.

The investigation of Benedict and Benedict in 1933 is typical of a group of experiments in which the energy consumption in carrying on "mental work" was studied. They found that adding digits without the aid of pencil and paper required 3 to 4 per cent more energy than was used at rest. Even this small amount could not be taken as the amount of extra energy consumed in the central nervous system, but rather the extra amount consumed in bodily movements and muscular tension involved in posture, etc., under test conditions in comparison to "rest." This means that the extra energy involved in the activity of the central nervous system was exceedingly trivial.

Goldstein (1934), working in this field, was not content with the flatly negative conclusions and suggested that it was the activity of the brain that was crucial in the situation, and that since brain activity could not be isolated the extra energy used in mental work was not measurable. The implication of the Goldstein suggestion seems to be that, were the suggested intense activity of the brain measurable, the differential would indicate that at least in this tissue there is considerably greater energy expenditure in mental work than at "rest." Since the brain is slight as compared to total body tissue, its activities could not influence the total percentage very greatly.

To some psychologists, including the author, the insistence that it is only neural activity that must be considered as underlying "mental activity" is not warranted. Furthermore, trying to relate intellectual activity to energy expenditure is a very fond hope indeed. To succeed in this would be to arrive at some sort of *coefficient of conversion* between mind and matter. To make the attempt, however, is consistent with the energistic assumption underlying the conventional mode of defining and evaluating activity and fatigue.

A fourth assumption underlying fatigue experiments pertains to the locale of fatigue. It is presumed that *fatigue may be either general throughout the body or specifically localized.* In some situations general fatigue is presumed to develop, and in others, fatigue is

supposed to pertain only to specific tissues. This distinction may have arisen from subjective considerations, for it is well known that sometimes the "feelings of fatigue" pervade the whole body, and at others, only a local member becomes uncomfortable and unable to function as usual. The actual experimental basis for the concept of localized fatigue rests upon evidence from two kinds of experiments: those upon tissues isolated from the body, and those in which a given member is exercised until its activity diminishes or it fails to respond

Reid's (1928) experiments with serial induction shock to a limb, both with intact circulation and with ischemia,[2] are an example of the second. Under ischemic conditions excitation results in almost complete exhaustion of transmission across the neuromuscular junction before any diminution in the contractile ability of muscle can be detected by direct stimulation. Ischemia apparently does not initially affect the contractile response under voluntary effort. Later on a pronounced failure develops in which case results of peripheral stimulation undergo less change than those of voluntary effort Even in a limb made ischemic experimentally, fatigue in the course of serial contractions is attributed largely to the central nervous system The earlier central failure under ischemia, as compared to that under intact circulation, is attributed to afferent inhibitory impulses from the active muscle rather than to local nutritional deficiency. Such impulses are supposed to arise in nonischemic preparations under usual conditions and also to induce central inhibition. In line with this, it was shown that recovery after "central nervous" fatigue has developed is hastened by removing the load from the muscle.

Reid's experiments are but one set which have been conducted under conditions that indicate that after a subject is no longer voluntarily able to contract limb muscles, electrical stimulation of the nerve supplying the muscle will still induce contraction. This, too, can be made to fail by continued application of a series of shocks [3]

[2] Ischemia is a kind of local anemia, generally in which the circulation is locally interfered with, as by pressure.

[3] Electrical stimulation used on nerve and muscle tissue is of two kinds. Continuous, or galvanic stimulation is the application of a steady current. Very brief applications are condenser discharges, or faradic stimulation The latter are generally called *shocks*, in nerve physiology.

at too high a rate. After contraction failure has set in, direct stimulation of the muscle itself will result in contraction. From such results, three kinds of fatigue have been named: voluntary fatigue, attributed to the central nervous system; transmission fatigue, localized in the nerve-muscle junction; and contractile fatigue, a property of the muscle itself. In some cases even more detailed analysis than this has been attempted, but we need not examine this work here. The three-locus classification is sufficient demonstration of the effects isolatable on the basis of diminution or cessation of response, and which therefore are called fatigue.

General fatigue is the type that is usually assumed to develop under the ordinary everyday conditions of mild activity, and during loss of sleep regardless of activity. Tests of activity made at various times of day indicate that activity rate varies from one part of the day to another. If productivity, therefore, is to be taken as the evidence for the presence or absence of fatigue, it may be said that fatigue is not equal throughout the day and recovery does not await sleep or periods of complete inactivity. Under common views of explaining fatigue and restoration, this fluctuation is not too well accounted for.

Explanations vary from assuming recovery during rest periods such as lunch hour to variations in motivation. It is to be granted that voluntary effort is a factor opposing the slowing down of productivity but not necessarily opposing fatigue. If fatigue is actually a metabolic affair, motivation involving an increase in activity can at best only mask it. If, however, it is admitted that anything can *mask* fatigue by inducing the productivity to remain high, then productivity can no longer be taken as an absolute indicator of presence or absence of fatigue. Such reasoning introduces grave difficulties into experimentation on fatigue.

An answer to the objection regarding the role of motivation has never been given. All that has ever been done is to attempt to control incentives and motivation, it *being supposed that if these are* constant then the development of fatigue can be charted.[1] This is not necessarily so, for the kind of motivation used has been found often to forestall the appearance of many of the phenomena included in the fatigue picture found outside the laboratory under similar conditions.

Inherent in fatigue experiments is assumption five—*that if fatigue occurs, it can be detected and measured.* This is so basic that perhaps it should have been mentioned earlier in the list of assumptions. More specifically, it might be stated that if fatigue occurs it can be detected and measured by the procedures conventionally in use. It would seem to be nothing less than a general axiom to assume that what one deals with is *detectable* if not measurable Otherwise the matter belongs outside of science.

Some years ago, Johnson (1929) was interested in measuring the impairment (fatigue) which develops in the course of a day's ordinary activities. To do so, he chose a specific task, accomplishment of which could be measured. Performance in this task was intended to indicate the general fatigue which was supposed to develop during the day Poor performance in the task would indicate fatigue. Good performance would indicate little or no fatigue The task was a simple intellectual affair such as number substitution. It was given to a number of college men one-half hour before retiring at night, and also one-half hour after arising in the morning. It was assumed that after arising in the morning the individuals would be the least impaired, hence performance then would be the best reference by which to judge performance at night, after the day's fatiguing effect. The test was continued for some weeks The outcome, however, was unexpected. Most, if not all, of the subjects did better in the tests at night than in the morning. This was in spite of the rest and recuperation they were supposed to have obtained during their night's sleep

Johnson's study is far from being alone in showing just this sort of result It is clear that Johnson assumed that at least some fatigue develops during each day. He assumed that it would be manifested in the difference between morning and evening performance. The latter performance was expected to be poorer than the former by some appreciable amount. He certainly did not expect evening performance to be better than morning performance. If this were to eventuate, he would have to interpret it as an indication that fatigue improves performance. That fatigue improves performance is a conclusion that few investigators would be willing to entertain. It must be accepted, nevertheless, if the premises just

mentioned are held. That is, it must be accepted if one maintains that fatigue occurred and can be measured, and that the tests used measure it. It ought to be obvious, however, that neither Johnson nor anyone else must restrict himself to these assumptions.

Johnson (1939) has been foremost among those who find fault with common ways of detecting fatigue and measuring it. In so doing, he points out what should be, but is not, somehow very obvious. He reminds us that the organism is very flexible in the way that it does things. It functions to accomplish ends. As it proceeds, an end may be accomplished at one moment by one means and later on by a different means. This, Johnson calls *compensation.*

What is conventionally done in "measuring" performance in order to detect and quantify fatigue is simply to observe one of the means to an end. Arbitrarily, the one means is taken as *the only* performance to be observed. Since few tasks are so arranged that an objective is precluded in performing them, the individual is generally involved in reaching a goal. As activity progresses, he becomes unable to continue acting exactly as he did when he started out. So, in order to continue at all, he modifies his behavior. In this case, either the original goal, which was reachable by more than one route, or the goal of merely keeping on with some kind of activity is still motivating the subject. So he continues on although his specific performance changes. If the experiment has been so rigidly set up that the new behavior, in being different from that required, lies outside the method of measurement in the experiment, it is as if the subject had become "exhausted." The record shows he has come to the end of the kind of performance required. If, on the other hand, flexibility of performance is allowed by the provisions of the experiment, the shifts in the *mode* of performance are missed entirely as they occur and therefore are not taken as evidence of fatigue or impairment. In either case, *compensation* is not presumed as a possibility, detected when it occurs or used as evidence of the subject's condition, even if noticed.

As Johnson points out, and as should be obvious, impairment (fatigue) is evidenced in the fact that the organism cannot continue to perform in the original manner. Changes in mode of performance should be observed and something useful should be done with such

data. Without observation of the changes in mode of the subject's performance, the usual technique gives no forewarning of an eventual breakdown so long as accomplishment is up to par. There is no assessment of the cost to the individual, hence no anticipation of what later appears to be a sudden exhaustion. Since no provision is made in the typical experiments on fatigue for recognizing compensation, it may be said that *unwittingly it is generally assumed that compensation does not occur in human task performance.* This can be called the sixth assumption.

An outgrowth, if not a corollary to the assumption that fatigue may be a general state and thus affect all performance, is the seventh assumption that *fatigue induced by one kind of activity may be measured by examining performances in an entirely different task.* This assumption has been used many times. The following investigation is a typical and large-scale example.

In 1934, a study of fatigue in truck drivers was reported by the U. S. Public Health Department. A number of scientists were consulted in its planning, for it was to be an important undertaking. The specific aim of the study was to determine the relation between the hours of driving, i.e., since the drivers had a "major sleep," and deterioration in a battery of laboratory tests. This procedure was chosen in preference to what would seem to be a more direct attack on the problem of fatigue.

A total of 889 drivers were tested. They were classified into four groups, in accordance with number of hours worked per week, and into three categories, depending upon the number of hours driven since a major sleep period. The men were also classified into age groups.

The tests included simple reaction time, flicker response, body sway, manual steadiness, vigilance (steering efficiency combined with brake reaction time), diastolic blood pressure, tapping, and strength of grip. The term *fatigue* in this study was taken to mean a "psycho-physiological" state inferred from deterioration on a majority of the tests taken. No single test could alone be the sign of fatigue. Deterioration in several tests had to be evidenced.

The results showed that the greatest correlation between test performance and hours of driving since major sleep was manifested

in the older men. Except for tapping speed, the most marked differences in performance were found among the older men before and after long hours of driving. This was spoken of as an age trend. Some tests, however, were performed even better after a number of hours driving than before.

If we are to assume that what the U. S. Public Health Service was really interested in in the first place was the question of the relation between long hours of driving and accidents, it was curious that a more direct study of this relation was not undertaken. In connection with number of hours work since "major sleep," reports on the feeling of tiredness and other forms of discomfort might have been obtained. These two factors could have been related to the number of accidents while driving. Although in this procedure, as in the procedure actually used, the existence and nature of fatigue would have been left undecided, a set of results more pertinent to the original questions might have been found

As it turned out, the general conditions (long hours of work) under which the subjects showed deterioration in performance in some tests were those in which they showed improvement in others Were the experimenters to have faced this result squarely, they would have had to conclude that the experiment had been simply a screening process to determine which tests were positively correlated with hours of work and which ones were not

Regardless of what we may say about the procedures and the relevancy of the answers obtained to problems of fatigue, the investigation is a very good example of the employment of the last assumption mentioned It is the assumption that fatigue is a general condition which can be detected in performances other than those inducing it

Our chief concern in this example is not the many details of outcome It is rather with the logic that underlay the whole experiment involving many subjects, considerable time, and much money. The outcome did, without doubt, throw into discard the idea that the result of prolonged activity on one task alters subsequent activity on every other task, for the worse. If we assume that the results of prolonged activity in this case include general fatigue, then

general fatigue does not impoverish performance in all other specific tasks. It must be said to improve some of them.

Whether any given individual will accept this conclusion at face value depends upon what kinds of assumptions he is willing to make.

In line with the assumption that fatigue is an energistic phenomenon, *the slowing down of processes is taken to be an essential manifestation of the existence and development of fatigue.* That *the diminution of process is the essential fatigue indicator* may be considered the eighth assumption underlying conventional views on the subject. Diminution of performance ("work decrement") has taken such a dominant place in the thinking upon the subject that it is often a *sine qua non* for the presumed existence of fatigue. Even when other kinds of performance-changes are used as indicators, such indicators are taken to be the over-all manifestations of complex mechanisms within which some one or more elemental processes are slowed down or terminated. This assumption possibly stems from work on isolated mechanisms. Isolated nerve tissue from both warm and cold-blooded animals can be studied for prolonged periods outside the body. Diminution of response may occur under the application of drugs or various salt solutions. Much of this, at least for a time, is quite reversible, and the idea of fatigue is seldom applied to it. Tissue also becomes refractory for short periods of time as a natural part of the activity-rest cycle. But, if change of response occurs, under repeated or prolonged "stimulation," the change is generally labeled fatigue. The most characteristic change under prolonged or repeated stimulation is diminution. Apparently this is taken as the prototype for fatigue in prolonged activity of the intact organism.

Accordingly, the aspect of any performance in the human subject which is singled out for study in a fatigue experiment is one whose diminution in rate, size, or amount, etc., can be measured. For example, in ergographic experiments in which the flexion of a finger lifts a weight, fatigue is measured either by the reduced amplitude of the flexions or the reduced rate at which the flexions occur. The amount of discomfort or anything else that develops during the course of the series of flexions is not taken into account in the measurements.

Man's interest in studying work[4] has centered mainly on the conditions under which work slows down or becomes impossible. A great many studies have been made on this problem. The study of *work decrement* usually carries with it assertions or implications regarding *fatigue*. Sooner or later fatigue is brought in as a principle to explain work decrement, or else work decrement is used as a sign of fatigue. In only rare instances has the general use of this label been disavowed. Robinson (1934) has suggested the avoidance of the term fatigue in certain connections. Watson (1919) and Muscio (1921) were others who held to the inadvisability of using the term Of the many studies of fatigue via work and work decrement, the following are examples

First, there have been many ergographic studies. These are studies of muscular movement against resistance Most usually a limb or some part of a limb such as a finger is used Finger flexion, for example, is made against spring tension, or to lift a weight. Successive flexions are recorded graphically By this means the diminution of amplitude of flexion is thus made measurable. Amplitude decreases from flexion to flexion The experiments often continue until the fingers can no longer contract It has been found that the arrival at zero amplitude does not represent exhaustion, for the immediate substitution of a lesser resistance or weight allows renewed contraction although at not so great an amplitude

Ergograms present considerable variety of form Some of them show steady progressive decrement in flexion from near the start Others show fairly well maintained amplitude until at last a sudden rapid decrement to zero sets in. Still others are irregular Robinson, for example, pointed out that the form of the ergogram is significant in that its variations indicate that chemical factors such as the development of fatigue products are not to be considered the sole basis for work decrement Be it known, nevertheless, that he and

[4] Work and activity in general, in psychological literature, seem to be almost synonymous terms Nevertheless some writers recognize that work, strictly speaking, is a physical term signifying transformation of energy. Work is the action of physical force on mass against resistance This use of the term, *work*, in both its physical sense and its looser sense in a purportedly scientific discipline such as psychology is unjustified It is only one of many examples of the prevalent looseness with which technical terms are used

most others would seek explanations in body chemistry before resorting to a search for other origins. The persistent character of the differences displayed by different individuals from day to day indicate not only that fatigue products are not the sole basis for work decrement, but that the other factors, whatever they are, remain quite constant in the individuals. Numerous investigators have had to admit what they call psychological factors and that these have prevented them from coming out with simple conclusions regarding even such restricted performances as ergographic weight lifting under laboratory conditions. In ergographic performance, with sufficiently light loads, activity may reach an equilibrium which can be maintained indefinitely. In such cases, this equilibrium is a product of load times rate of flexion.

A second type of study of fatigue as evidenced in work decrement involves the contraction of muscle without an experimental resistance such as weight. In ergographic performance, decrement usually sets in early, but in contraction without load complete decrement seldom occurs.

A third set of studies pertains to the over all muscular performance of the individual either in treadmills, etc., or in natural activities such as running, swimming, rowing, climbing.

A fourth group of studies has had to do with periodically elicited reflexes.

Whereas the studies of muscular activity just mentioned are more or less rhythmic performances in which repeated contractions at a maintained amplitude are demanded, a fifth kind of study has had to do with performance in which *organization* of movement was the aspect singled out. Even here the concept of *decrement* was still employed. One of the very best studies of this kind was Dodge's (1917) investigation of eye movements. The eyes were made to move horizontally through a 60 degree arc. Two upright rods each 30 degrees from the central line of regard were successively fixated. According to instruction, the movements were to occur as rapidly as possible in keeping with the subject's ability to make successive fixations of the two rods. Some of the typical phenomena appearing in this performance were: (1) reduction in speed of movement; (2) reduction in accuracy of fixations; (3) irregularity in the line

of horizontal movement. These phenomena, especially the latter two, indicated to Dodge something more than a mere likeness to the usual ergographic experiments. His work was unusual in that his findings were interpreted as a *disorganization* of reciprocal innervation. Thus disorganization rather than mere decrement was taken to be the basis of the changed performance.

A kind of protracted response indicating either disorganization or periodic decrement to zero depending upon the interpreter's viewpoint is that which is represented in *blocking*. Bills (1931) presented visual items one at a time at a constant rate. The subject was to respond by naming them. Actual errors were few, but hesitations and failures to respond within the time allotted were many. These were called *blocks*. Blocking occurred not only in letter- and color-naming, but also in other tasks such as voluntary reversals of perspective, substitution, giving opposites, and in continuous addition or subtraction. The blocks tended to bunch together. A wave-like alternation between many and few blocks was typical. Errors occurred consistently in proximity to the blocks, suggesting a common basis for the two.

According to Bills there were two contrasting effects in connection with blocking. Practice reduced the frequency of blocking and fatigue increased it. Blocks were considered expressions of fatigue and the phenomena gave rise to the idea that a refractoriness was responsible for the blocks.

Bills also attributes to blocking the function of protection against more serious decrements in performance. This is a concrete example of a conventional attitude toward fatigue. As such it may be stated as the ninth quite general assumption in regard to fatigue. That is, *fatigue is assumed to be nature's way of protecting the organism against damage*. This is a gratuitous assumption, since it seems to hold no integrant place in any systematic formulation of human behavior. Nevertheless, it is a rubric that is often announced with regard to fatigue.

Our next problem concerns what has been commonly assumed as the *basis* for work decrement. It was stated in the second assumption listed above that fatigue is generally looked upon as a matter of body chemistry.

As early as 1865, Ranke proposed that fatigue substances develop during the course of muscular activity and that these operate to bring about work decrement. He proposed these substances because he could not conclude from his experimental results that activity used up the energistic resources of the active tissue. Since Ranke's day many have applied themselves assiduously to the isolation and identification of these hypothetical substances.

A summary that appeared about thirty years ago (Scott, 1918) held that the hydrogen ions of certain substances such as lactic acid underlie fatigue. It stated also that other positive and negative ions were responsible. Along with these, certain products of protein disintegration and substances such as purine bases, uric acid, etc., were included. This summary denied, however, the existence of specific fatigue substances such as had been postulated previously. Although it could be supposed that decrement-producing substances, if they exist, could act on tissues in one or more ways, their existence has not yet been assured. Such substances might have direct depressive effects on muscle tissue; they might affect sense organs, giving rise to discomfort, or they might induce what we now label "feelings of fatigue" by bringing about relaxational innervation and limpness.

It is to be noted also that the psychological literature contains attempts to express *laws* regarding work decrement which are quite similar to those pertaining to learning and recall. For instance, Robinson (1934) stated the following a few years ago.

1. "Work decrement of an *S-R* (stimulus-response) connection is relative to the recency of the previous functioning of that connection." This is called also the law of refractory phase. In so far as the law implies that an early repetition of a present performance is less likely than a later one, as far as the organism tendencies are concerned, it is on solid ground. Undoubtedly the idea of refractoriness may be used here as a generic term, but since the attempt is to connect it with nerve function, it is misapplied. Refractoriness in nerve pertains solely to the short period after the cessation of the axon spike potential. We have already cited the experiments of Thorndike on the varied response to spoken words. The experiment included not only presentations every 2.5 seconds, but also a series in which the rate of presentation was every 5.0 seconds. It

was found that the tendency toward repetition was less with the shorter intervals than with the longer. This illustrates the tendency to avoid repetition, but although taken to be a case of refractoriness it hardly answers the nerve physiologist's requirements of that concept.

2. The second law that Robinson stated was that "The work decrement of a given S-R connection is relative to the frequency of the previous functioning of that connection." That is to say, decrement occurs as a consequence of continued or repeated action. This is another way of stating the very first assumption given in the present chapter.

3. "The work decrement of a given S-R connection is relative to the connection existing between that S and other R's." This principle was evolved to express the principle of competition and is illustrated by a case in which S has come to be able to elicit R and R_1. If the R connection is slightly stronger, it will be elicited in preference to R_1. But if repeated presentations of S occur, the connection with R weakens and finally R_1 is elicited instead It is easy to see the two sides of the situation—not only the decrement aspect but the other which is the learning aspect. It may just as well be said that the organism has, in this case, learned to respond to R_1 Thus, this is a law of learning as well as of fatigue

4. "The work decrement of a given S-R connection is relative to the strength of the specific connection." This principle indicates that if the connection is weak to start with, little competition is needed to weaken it to the point at which other responses will supplant it.

5. "The work decrement of a given S-R connection is relative to the qualitative integrity of the S throughout the work period during which the decrement develops." This principle takes cognizance of the possibility that a given S may or may not be the same throughout a prolonged work period. The influence of the changed conditions is often in the direction of increasing decrement The new S condition may represent the addition of competition in response and the elimination of others, or a change in the strength of connection between S and the desired R.

6. "The work decrement of a given S-R connection is relative to the quantitative constancy of S throughout the work period dur-

ing which the decrement develops." The continued operation of an S-R connection may result in a decrement so long as S remains at a certain intensity, but the decrement may be increased or lessened when a change in the intensity of S occurs.

7. "The work decrement of a given S-R connection is relative to the decrements that have developed in other S-R connections." This is a recognition of the possibility of a transfer of decrement from one connection to another.

It is to be noted that these so-called laws do not take directly into account organization on a level which would include directionality, alignment, and continuity aspects of behavior; therefore they would seem to be inadequate to account for human behavior as we know it The assumption tacit in all studies which espouse the S-R laws is that there is a set of principles which are separate and distinct from those that have to do with the origin and expression of specific alignments in the organism called purpose, motivation, etc This is true of virtually, if not all, conventional fatigue studies as well This assumption implies that the one set of principles pertain to activity of tissue and that the other set, if admissible at all, pertains to the alignment functions just suggested. It is the aim of such theorists to determine how the one set of principles operates before attempting to superimpose the second set. This is a denial of organismic unity. Attempts to develop a concept of functional unity within the organism repeatedly crop up, but without any effort to cast off the dualism just alluded to.

A tenth and final assumption which should be called to attention is the conventional adoption pertaining to kinds of fatigue. According to this view, there are *three kinds of fatigue* -each so different from the other that the three must be handled quite separately (Bills, 1943).[5]

The first of the three kinds of fatigue that is generally mentioned is called objective fatigue and is simply that which causes work decrement. Work decrement is presumed to be the measure of it.

[5] In addition to these, new "kinds" of fatigue appear from time to time in the literature. They have been more common during the past decade than previously. They seem to occur without any broad theoretical foundations to serve as a justification. There are, for example, "combat fatigue," and "convoy fatigue."

In this sense it has the appearance of objectivity, but the matter does not rest there. Since not all work decrement is due to fatigue, fatigue is left for those forms of decrement which have not already been given other names. This matter was looked into by Forbes (1941) and his colleagues in their studies of peripheral nerve activity. They concluded that all of the various "slow-down" manifestations were forms of fatigue, some of them very primitive.

Furthermore, those who have used work decrement as an indicator of fatigue have sometimes not stopped at this point but have made the decrement and fatigue synonymous. In doing this the performer has been excluded as an object of scientific consideration. To study fatigue, work products are the center of attention. Fatigue is merely the amount by which productivity drops in unit time. This procedure rests on an industrial or "practical" definition of fatigue, which is not a biological definition of fatigue at all. It involves nothing that is either psychological or physiological, or nothing that is "personal."

The second kind of fatigue has been called *physiological* fatigue. This fatigue has been defined as a change in the behavior of some physiological mechanism. Although this change is very often a diminution of function, the change may be manifested in some other way. For example, the size of the pupil of the eye is supposed to increase as the work week progresses. The number of eye-blinks per unit time has been taken as an indicator of fatigue in reading. While the use of eye blinks as a fatigue indicator has been disavowed by some investigators, it is a common example of the use of a physiological mechanism for the study of fatigue. It is also an example of the fact that the validity of the indicator is as much in doubt as is the nature of the fatigue that is being indicated.

The third kind of fatigue is called *subjective* fatigue. This is merely the feeling of tiredness. As an experience this kind of fatigue is never in doubt. Although it could be used as a more certain starting point than any other, it has been studied very little. When no simple relationships between feelings of fatigue and rate of performing tasks are found, the investigators have tended to give up, thinking that they had followed the wrong hunch, or at least a very unfruitful one. One of the better known instances of attempting to

correlate feeling with performance was the study by Poffenberger (1928). He found no consistent relation between the degree of tiredness reported and the time of quitting the task. This might have been expected, for the factors underlying the subjects' activity include more than their feelings of tiredness Further progress might have been made had certain other major factors been included in the study. Were it to be found that there were too many to handle, the experimenter still would not be compelled to regard the feelings *of tiredness as a fickle and irrelevant matter The problem would remain just as legitimate as if a simple fixed relation between feeling tone and point of stopping work had been found.*

All investigations aimed at interrelating the phenomena in the three categories have met with failure. No consistent relations have been established. No strict predictions from one set to the other have been achieved. It is now seen that to treat fatigue in one way may be to ignore the others On this account one could just as well utilize three separate labels in referring to these phenomena. As was previously pointed out, dropping fatigue as a common name for these phenomena has already been proposed

There seems to be, however, a willingness to leave matters as they are. In this state of affairs, some individuals treat fatigue either as axiomatic and not in need of being defined, while others take it as a narrow phenomenon to be defined "operationally" in terms of the restricted conditions used in a particular experiment. In this way Elmer Brown's fatigue is bound to be somewhat different from the fatigue in Oscar Jones' investigation. They say, "Why not, aren't all concepts to be defined operationally?"

No one examining the assumptions we have listed regarding fatigue could fail to see that many of them are untenable in light of the experimental outcomes in the studies which have utilized them There is nothing in them that tells us what fatigue is when we are measuring it, unless it be the much used idea that work decrement is fatigue's infallible indicator. Even so, those who use work decrement in this way do not go any further than to measure work decrement under some specific set of conditions. They generally stop at this point without making *the organism* the object of analysis. Fatigue tests based on the assumptions as listed have not elicited wide-

spread conviction. There is always obviously something wrong with them. If such tests have had some use, it is largely because there has been nothing better offered. At other times experimental attention to them has been mainly for the purpose of improvement.

For those who are not content with the fatigue picture as it is found here there is opportunity for reconstructing it. An entirely different one may be developed. A scheme may be set up which is worth testing (Bartley & Chute, 1947). This scheme ought to be an incentive for investigations not found up to now. Before beginning on a theoretical structure, let us stop making fatigue and work decrement synonymous. A second step might be the return to the man-on-the-street's outlook to gain fresh bearings. The layman knows the existence of fatigue by his own feelings. Actually when men first began to study fatigue they were attempting, for a little while, to answer questions regarding what made people feel tired The original problem surely centered around the conditions for the development of fatigue as a personal state. Call this state psychological, if you will. This will not matter just so long as one adheres to the original problem, that fatigue is a state of the individual in which he has an aversion from activity of some sort, and feels uncomfortable and inadequate in a unique way which he can always identify Scientific investigators did not approach the problem with this in mind, but quickly seized upon the hunch that an individual became tired because he lacked energy, and experiential matters were irrelevant. In accord with this they sought ways of studying him as a depersonalized energy system—a machine without a history, and without impelling direction

If we even sketchily examine the experience of fatigue we shall find certain items from which useful deductions can be made. Some of these may seem to border upon the commonplace while others seem unusual enough not to have been utilized as guides to experimental planning.

Analysis shows that when we are tired we are tired of *something.* Some persons are "tired of everything," but to be tired at all, one must be tired of something. This is more than a mere dictum of logic; it can be verified. Fatigue, therefore, has a reference. The reference is not the energy consumed in activity but a "task," which

to the performer beginning a task has qualitative attributes. He feels some way about the task, and this attitude should be a major consideration of the experimenter. Since the immediate conditions of the fatigue lie in the performer's attitude toward the task, it is only indirectly that the task as a piece of mechanical activity is to be brought in. Although this attitude may arise partly from purely physical conditions imposed upon him, these conditions do not function in the fatigue picture until they pose a thwart which is recognized in terms of discomfort, or the inability to perform as wished. *The individual takes such matters as factors unfitting him* for continuing activity. The situation builds up through the conditions of the moment paving the way for more effective thwarting conditions in the next. The individual attempting to continue in the face of discomfort and other forms of unfitness is considerably frustrated. It is this frustration that gives rise to the bulk of the over-all unpleasant features of the situation we label fatigue. Thus fatigue is a form of frustration behavior

If we are willing to accept this description as a set of premises, an alternative scheme may be set up as follows·

The first assumption in this scheme is that *two different kinds (or levels) of events may occur within the organism* One kind of event is *intracellular change* The second kind is a change in *intercellular relationships.* The former we shall call impairment The latter will be called disorganization. Among instances of the former are changes that happen in the cell when deprived of its fuel or nutritive materials obtained from the blood stream Experimentally this kind of change can be brought about very definitely and under control. Anoxia is one form of impairment and is brought about simply by oxygen deprivation. Disorganization of the individual can be brought about in many ways

The question which might arise in the minds of some thinkers on the subject would be whether disorganization ever occurs without impairment as its basis. It would seem that the neuromuscular system of man is so versatile in its moment-to-moment intercellular relations as to give rise to any one of a variety of experiences including fatigue, when no detectable cellular functions lie outside the limits of normalcy. If it were someday to be shown that intracellu-

lar changes do take place in certain cases of intercellular disorganization, it might be equivalent to saying that we had gone within the cell for our basic *unit* of organization. We could still maintain the distinction between effects that arise from intra-unit difficulties and those which arise as special inter-unit relationships which we ordinarily call disorganization.

A number of phenomena are known for their over-all manifestation. Among them are, for example, anxiety and fatigue. These can be thought of as forms of disorganization. Just as psychologists do not immediately look within tissue units (cells) for the explanation of the syndrome of anxiety, we need not look there for the immediate explanation of fatigue. The view that we are developing puts fatigue in the same broad category as many of the clinical pictures with which psychologists are willing to deal on an organizational basis.

The second assumption is that *fatigue, one of the several possible forms of disorganization (an intercellular event) may occur without impairment (an intracellular event), and that impairment may develop in some forms and without the development of fatigue.*

The following empirical examples are evidence of the existence of each of these possibilities. As has already been mentioned, anoxia is a frank form of impairment. In anoxia a number of overt behavioral changes ensue, although at no time during the course of progressive anoxia need the subject feel tired. In such cases impairment occurs without consequent fatigue. McFarland (1938) found four different behavior outcomes in one of his oxygen deprivation experiments: (a) excessive amusement and bursts of laughter; (b) extreme irritation and roughness toward the apparatus used in the experiment; (c) perseveration and little awareness or care for poor task performance; and/or (d) quite early abandonment of the task in the experiment.

Under *some* conditions fatigue does develop with anoxia, but only under slow onset and when specific conditions present thwarts to the individual which he in turn recognizes. This is the case in mountain climbing when anoxia is slow in developing and where motivation is high and exertion is extreme. The impairment in

such situations frustrates the individual, whereas in a laboratory situation the motivation is less and the anoxia much more rapid in its onset.

In contrast to the anoxia example, the individual may become fatigued very quickly without any impairment as a basis. For example, he may be feeling quite well and "rested" until confronted with a very odious task or a futile situation, then he very quickly develops an intense feeling of fatigue. Fatigue may disappear rapidly when the appropriate social situation develops. These sudden changes are not accounted for by any intracellular changes about which we know. Despite the quick come and go of the fatigue and its apparent lack of connection with energy expenditure, we have situations here which satisfy the necessary criteria for fatigue. The fatigue is perfectly real under these circumstances.

The third assumption is that the peculiar form of personal disorganization called *fatigue arises in a situation in which activity is occurring against odds recognized by the performer himself. The odds are thwarts and when they operate the individual is said to be frustrated.* The initial odds are those of beginning discomfort, self-observed slowing down, self-recognition of mistakes, or other evidence of poor performance. The feeling of aversion develops and a sense of inadequacy and futility eventuates. One condition leads to another until the attitude becomes accentuated. Then there is no doubt in calling it fatigue.

The fourth assumption is that *the performer alone is the judge of his fatigue.* Observers may note inadequacies in performance of which the performer himself may be unaware, or the performer may find himself becoming uncomfortable when no sign of it has yet appeared in the manner of performance. When observers note slow-downs, mistakes, and qualitative changes in the manner of performance, these are to be treated like any other overt manifestations, but should not be labeled fatigue. They can be taken as evidences of change in functional organization for which further means of analysis may need to be devised. It is not until the performer's own recognition of his state begins to make him uncomfortable and inadequate in his own eyes that fatigue can be said to arise.

The fifth assumption is that *when odds are perceived as dangerous, anxiety develops, and when they are seen as uncomfortable or as thwarts to activity, fatigue develops*

A sixth assumption is that *the individual is not able to detect impairment in himself by the way he feels or sees himself perform* Impairment is a cellular affair and may be detected only by certain physiological methods appropriately devised for this purpose Not all physiological tests detect impairment Some of them deal only with disorganization of intercellular (organ or over-all) function

The severity of the malaise is no sure indicator that impairment exists The degree of discomfort at one instant is no indicator of what the feelings will be later, whether minutes or hours Hence any degree of malaise is no indicator of how soon or suddenly one may feel better fatigued no longer This is in contrast to what might be expected were impairment at the basis of one's feelings Disorganization of function (intercellular conditions) may give rise to all of the very distressing experiential symptoms to which man is heir Consequently feelings are no sure indicators of impairment Nor are physiological tests always adequate to make sure of the existence of cellular malconditions

There are still other assumptions which constitute a part of the present theory of fatigue Some may be considered primary and some secondary, or as corollaries They will be listed without distinction in this regard

Fatigue is not an energistic affair

Fatigue is not a localized phenomenon It is an essentially general state most aptly described as personal "Personal" here means that which applies to or describes the person When this label is used one is not called upon to distinguish, in traditional fashion, whether fatigue is physiological or psychological, nor even to posit how interaction between the two may occur, for no analysis into these components is used as a starting point Localized discomfort is simply discomfort When it occurs the individual may or may not be tired Such discomfort may act as a thwart, but it itself is not fatigue It, however, may be one of the causative factors of fatigue.

Fatigue developed in one situation cannot be measured under some other set of conditions by later employing these as a "test." There is no necessary connection between the two. If in some cases there happens to be a connection, it lies in the subject's common stance toward the two situations. It is only by investigation that this stance can be discovered. It cannot be assumed *a priori.*

Fatigue that develops on any occasion is not fully to be accounted for by the overt experimental conditions existing at the time. Fatigue is an affair of the organism and so must arise out of a temporal continuity. The direction of the organism in question must be taken into account. Just as these factors are used in dealing with other forms of behavior, they are to be taken into account here.

Since fatigue does not arise directly out of the expenditure of energy, *situations expected to be fatiguing must actually frustrate the individual.* Laboratory situations in which motivation is maintained at a high level by monetary and other incentives cannot be expected to be proper conditions for the study of fatigue. Such experiments are merely arrangements for seeing what comes of bodily motion or the exercise of other functions, quite apart from the real conditions which make for fatigue.

Efficiency

Under present circumstances there is not nearly so much to be said about efficiency as about fatigue. Outside of the purely metabolic studies a great deal of the work done on efficiency has been from the practical rather than the scientific standpoint Our discussion of efficiency has been postponed until now so that the contrast between the two viewpoints already developed for fatigue might be utilized with maximum accentuation.

In general the efficiency of a system is the ratio of output to input. When the concept of efficiency is applied to the physiology of the organism, energy and the physicist's definition of the term work immediately ome into use. Muscular efficiency is the type that becomes dominant. The input of the organism is measured in terms of the fuel (food) it consumes. Food has caloric value. And the relation of caloric energy to mechanical expressions of energy

can be understood in terms of a coefficient long familiar in thermodynamics[6]

The total useful output in a task may be divided by the total energy spent in the task to find percentage efficiency. Muscular efficiency is the relation between the total energy liberated in the body and the work output of the muscles.

Since the input is in terms of calories and the output is in terms of mechanical work, the output has to be divided by the thermal equivalent of the mechanical work. When this is done the gross efficiency of the performance is given. The net efficiency is the value obtained when the heat equivalent of work is divided by the *increase* in energy spent over that involved at rest.

A notable study of net muscular efficiency was conducted several years ago by Haggard and Greenberg (1935) in relation to the distribution of the day's food intake. These investigators were interested in the relation of net muscular efficiency when workers ate extra meals per day while keeping the total food intake practically constant. They found muscular efficiency rose immediately following a meal. It reached a peak about one hour after meals and then descended to the pre-breakfast level before the next meal, three or more hours later. If only two or three meals per day were eaten, the total time during which efficiency was high was considerably less when the same food was divided into more meals per day. They found that net muscular efficiency and blood-sugar level paralleled each other fairly closely. This being the case, an indicator of muscular efficiency is to be had in blood-sugar level.

Thorn, Quimby and Clinton (1943) found the blood sugar level and the metabolic rate in humans varies in accordance with the nutrient content of the diet. These factors vary as the intake is dominantly protein, carbohydrate or fat. If we are to take the findings at face value, it could be said that a high protein meal provides the highest and steadiest blood-sugar level and metabolic rate over a six-hour period following a morning meal. The high carbohydrate meal eventuates in the greatest fluctuations of blood-sugar level and

[6] In the c g s system 1 calorie = 4 187 joules or in the foot pound seconds (f p s) system 1 B T U = 778 ft lbs. The coefficient is therefore 4 187 in the one system of measurement and 778 in the other.

metabolic rate. With this kind of a meal, the blood-sugar level shows a rise beginning immediately. This is soon reversed into a fall below the fasting level existing when the morning meal was taken. The high-fat meal resulted in levels which were most of the time between those of the other two types of meal. The criticism with this study is that it does not seem to be a fair sampling. Only one subject was used.

The phenomena mentioned in these experiments are either those of basic body economy such as metabolism and muscle efficiency, or else quite tangibly connected with known features of physiology There are other fluctuations which are not nearly so closely related to known body mechanisms. For example, there are fluctuations of mood that are not accounted for by closely associated variations in known bodily functions. Such variations of mood certainly could be classed as having to do with over-all efficiency. Either the individual in such moods does not do so well as usual, or else in order to perform as well must try much harder. He is working against some sort of odds. Rhythms in mood involving 4 to 6-week periods have been reported by Hersey (1935), which fall into this category.

Efficiency is sometimes measured by the economy of motion in performing a mechanical task. The efficient worker is one who performs a task with the fewest motions and, within limits, the shortest ones. In factories task procedures are designed so as to minimize motion. This economy represents a kind of efficiency but not efficiency as we shall define it. It represents only a task modification and is not directly related to cost of activity to the individual. It is an externally imposed change in conditions and can be considered totally apart from what goes on *in* the individual.

Ryan (1947) points out that efficiency is very often confused with rate of performance. This would mean that a rapid worker would be considered more efficient than a slower one. This, of course, does not take into account the energy used in the two cases. In all kinds of tasks, some variability in performance occurs. Weinland (1927) studying variability in work curves in ergographic experiments concluded that variations represented lack of control rather than exhaustion of muscle tissue. Supporting this conclu-

sion was the fact that as work progressed, the subject was less and less able to keep pace with a metronome. Such variations are measured in terms of output and thus may represent fluctuations in efficiency, regarded as rate of doing work. As to their being variations in energy consumption, the matter is not so clear. If variations become very great they may become factors leading to fatigue. Whether this happens does not depend strictly upon the energy involved, but upon how the variations affect the individual's notion of success, etc.

Neither of these ways of considering efficiency nor the muscular efficiency which was mentioned first are the kinds of efficiency in which we are most interested. There is a far more pertinent matter that must be covered by the term. *It is the cost of activity to the performing individual.*

For a given individual the output may be little or it may be great. Examination may reveal great economy of motion or none. It may be shown that muscular efficiency is high or low, but still the matter of cost has been left untouched The efficiency of *workers,* whether doing mechanical work or intellectual tasks, is to be understood in terms of what the work costs them This cost is not directly and exclusively an energistic one. It is represented by a variety of factors and has to be dealt with by a variety of methods.

Ryan states that the cost of work to the individual includes such items as fatigue, time, effects upon health, social adjustment, and the ability to enjoy free time In short it may be said that the costs are all of those effects which militate against the maintenance of some standard the worker himself has set up, or against reaching some goal that the worker himself has his eye upon These costs have to do with the worker's relation to some scale or system of values which he has adopted. When factors, such as Ryan lists, operate against this system there is some adverse effect upon the worker which can be called a cost. Since there are no strict coefficients of conversion from one category to another, the student of the matter is left with some very serious problems to solve. If he must immediately deal accurately with concrete processes, then he may utilize one or the other of the ideas of efficiency which were first mentioned. But if he is willing to face the larger, very perti-

nent and exceedingly more difficult problem, he may seek some scheme with characteristics similar to the fatigue scheme we have just described, in order to provide a rationale for procedure. In doing this, he will avoid trying to impose some external measuring scale upon the performing subject, but will develop ways by which the subject will be the measuring device to express the costs which he is paying.

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CHAPTER 8

PERCEPTION

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Theoretical problems in the field of perception center around the question: How can we account for the structure of the perceived world with all of its symbolic, affective, aesthetic, and demand characters? Perception reveals orderly arrangements of objects and events in space and time, partly because the external world is itself orderly, partly because our own integrity as persons depends upon our ability to unify the deliverances of our senses Whether order is immediately given, as some claim, or built up by experience, as others maintain, is still far from settled In either case, it must be due to mechanisms operating in the present, although involving residuals from past experience, which it is the ultimate aim of science to understand. When stated in physiological terms perception seems simple enough, chiefly because so much is glossed over when we use general concepts to cover up our lack of knowledge of what actually goes on Thus it can be said that perceiving involves stimulation of sense organs, afferent neural conduction, central reception and elaboration, with perhaps efferent discharge followed by feedback from receptors, originally stimulated or secondarily involved, and from muscles, viscera, and glands which also take part Since perception is part of the business of living it is the whole organism which acts and reacts, not just this or that specific afferent sector. Stated in psychological terms, perceiving involves the apprehension of and reaction to the qualities and properties of objects and events as they interact with the organism. This account, too, of perception sounds simple but it raises many problems and to some of them we now turn.

Sensation and Perception

Since the days of Thomas Reid (1764) it has been customary to regard perception as made up of simple sensory and imaginal components. In Reid's terms, perception has an external object and involves a belief in its presence and reality. If we divorce the perception from its object, concentrating, as it were, on the pure mental content, we arrive at sensation. This is on a lower level than perception. On the other hand, we may conceive things not perceived. Some conception enters into every perception. Perception therefore involves fusion of conceptual (imaginal) and sensory processes. This theory appears in modified form in many different guises. Helmholtz accepted such a view when he pointed out that we are normally directed toward the perception of objects rather than to the sensory qualities revealed by analysis. As higher processes to explain perception he postulated judgments and unconscious inferences (1925). Wundt, recognizing there is "more" in perception than the simple deliverances of sense, formulated his principle of creative resultants to regularize the miracle of synthesis. Thus he wrote: "This principle attempts to state the fact that in all psychical combinations the product is not a mere sum of the separate elements that compose such combinations but represents a new creation" (1912, 163f.). The appeal to extra-sensory processes to explain perceptual phenomena for which there seem to be no stimuli dates back to the philosophical, pre-experimental era of psychology and continues on into the present.

Still another formulation of the relations between sensation and perception is found in Titchener's context theory. According to this theory, perception consists of a core of sensations aroused by the stimulus and a context composed of images and or secondary sensations contributed by past experience. The core is primary because it is in more or less close correspondence with stimulus and receptor activities and remains fairly constant from individual to individual. The context depends upon past history and may therefore vary from individual to individual. This theory gained wide acceptance because it seemed to offer a genetic account of meaning. Meaning in this theory springs from context and, since context de-

pends upon individual experience, the variety of meanings which a given stimulus can evoke, while arousing the same sensory core, in different individuals, seems to receive adequate explanation (Titchener, 1909, pp. 367ff).

In reformulating the context theory, Boring refers to the core as the basic sensory excitation that identifies the perception while the context "consists of all the other sensory data that modify or correct the data of the core as it forms the perception" (1946, p. 100). The context also includes "knowledge about the perceived object as determined by past experience, that is, by all the brain habits which affect perceiving" (p. 100). For Boring the core in the visual perception of size, for example, is the size of the retinal image of the object, all else pertaining to the distance of the object is context. The fact that an object at twice unit distance is not visually halved in size when the retinal image is (roughly) halved is due to the presence of distance clues which the brain integrates with the smaller retinal image "to make the perception . . . look as large as the perception from the larger retinal image" (p. 100). Hence context "increases the correspondence of the perception to the real object" (p. 101). But in Titchener's formulation, context accounted for individual differences in perception and the core accounted for what was common since it was closer to "reality" than context. Now with emphasis on object-properties, the concept of reality seems to have changed from one-to-one correspondence with stimulus to over-all correspondence with objects.

There are difficulties which must be faced in both Titchener's and Boring's core-context theories. First, we need to know how integration unifies core and context to produce the effects usually attributed to "constancy." Integration somehow makes objects look larger than they should look if retinal size alone determined perceived size. The mechanism by which this is done is not stated in the theory. Integration thus becomes the problem, if not the mystery, in place of the facts which it originally set out to explain. It is difficult to understand how integration can operate to transform core data in the many ways necessary to account for the properties of perception. Perhaps the difficulty springs from the assumption made in the core-context theories that perception consists of two

parts which must be synthesized to form perceptions. Since objects are perceived only as parts of extended fields, might it not be simpler to begin with this as a fundamental postulate and to devise a theory which derives the properties of objects from field-properties? That this may be done will be shown in a later section.

A second difficulty with core-context theories is in their assumption of "real" properties, whether sensory or object-properties are in question. While such object-properties as shape and size may have plausible claim to real status, the difficulty appears more clearly when the color of objects is in question. We know that the color of an object depends upon the spectral energy distribution of the light reflected from it and its surround and upon the state of adaptation of the eye. If the light from the object or from the surround or the adaptation is changed, the color of the object undergoes modification. Adaptation may compensate within limits for changes in the stimuli reaching the eye thereby approximating so-called color constancy. In every case the color of the object is just what it appears to be under the given conditions of viewing. There is no unique state of adaptation, no particular set of spectral energies according to which real color may be defined. Scientifically, our job is prediction. When the colors of objects can be specified under all conditions, "real" color will be seen to be an unnecessary, if not a metaphysical concept.

Similar considerations apply to form and size. Apparent form and size are what they appear to be and one visual set of conditions cannot be taken as any more real than another. Perceived form and size of objects should prove to be as predictable from a knowledge of the "space level" as colors have been shown to be predictable from a knowledge of brightness and chromatic levels (Judd, 1940). Thus the problem of constancy, which has been largely responsible for the postulation of real object characters in recent times, is seen to be part of the more general problem of field versus local determination.

A somewhat different and perhaps more functional account of perception in terms of cues rather than context is found in Woodworth. For Woodworth perception is essentially a form of awareness or behavior by which the organism makes its adjustments. The

organism utilizes various cues in locating and responding to objects, in reaching goals, in solving problems. Rats, for example, may use vision, smell, kinaesthesis, or any combination of these to run a maze without errors. Humans also use cues whether consciously or automatically. A small difference in time of arrival of stimuli at the two ears is immediately perceived as a single tone displaced toward the side of prior arrival. But if, through experimental manipulation, the time difference favors one ear and the intensity-difference is made to favor the other ear, the two cues will be in conflict and the observer will be uncertain as to which side the sound is coming from; or he may even hear two sounds in different places In general, however, cues and meanings fuse automatically and rapidly. "One does not first notice the cue and then get the indicated fact a cue is absorbed into the suggested meaning and is no longer observable for itself" (1938, p 595). Hence the barest sensory data can become carriers of knowledge, either true or false

The dynamic aspect of Woodworth's approach to perception appears in his emphasis on the stimulus as part of the response, not merely as a central process Stimuli do not break in on a resting, neutral organism; rather they break in on some activity already in progress, modifying it. The same stimulus may release different responses and hence evoke different perceptions (1926) Woodworth's theory shows how the sensory data of Wundt and Titchener can be related to behavior in a dynamic, functioning organism

Stimulus and Receptor Correlates of Perception

The context and cue theories of perception stress the part played by sensory processes in the perception of objects and in the genesis of meanings. Correlation of sensory dimensions with events in the external world and with receptor activities has been a main consideration in the context and cue theories Dimensionalism is the natural outgrowth of introspective analysis and the experimental tradition dating back to Fechner and Helmholtz It attempts to correlate discriminable aspects of perception with measureable properties of stimuli and as such was long known as the science of psychophysics. However in its modern form the chief concern is not with the rela-

tion of mind and body taken as two distinct realms of being. Rather dimensionalism aims first to correlate physical and psychological dimensions and then to show that they are identical (Boring, 1933). The psychological dimensions stemming from Titchener are quality, intensity, protensity, extensity and attensity. Since attensity or clearness is hard to define and perhaps normative rather than quantitative it has not enjoyed the same status as the rest. A dimension must be capable of variation while other dimensions are held constant according to Titchener. Present-day dimensionalists set up the criterion that a dimension must be kept constant while others are made to vary. In line with the latter criterion it is possible to plot contours for constant loudness, for example, while pitch or volume varies (Stevens, 1934).

At first sight it would seem that the program of the dimensionalist can be carried out quite easily, but it, like other approaches, has its difficulties. First, there seem to be more psychological qualities than there are physical dimensions, at least in vision, hearing, and the cutaneous senses. On the other hand, some conscious dimensions seem to be lacking, such as volume or extensity in odors, for example. In some cases there are no adequate local stimuli for vivid, extended sensory data, such as the complementary colors seen in dark parts of strongly chromatic fields. Nor can the sensory phenomena always be correlated with receptor processes or by known differences among them. This is particularly true in the case of the tactile senses where the correlations with end-organs have been very poor (cf. Dallenbach, 1939). The appearance of new qualities, presumably due to neural synthesis, like heat from stimulation of warm and cold spots, and oiliness from pressure and warm spots, makes the problem of correlation all the more difficult since the question arises as to whether or not the emergent qualities are simple. Nor do the well-established facts regarding the properties of nerve impulses make the problem of correlation any easier since the impulses in single fibers differ only in rate, frequency, and amplitude, which provide only three physical dimensions to account for the many more in perception. Even when the functions of synapses are invoked there is still not enough information on the neural side to account for all sense qualities.

It is conceivable that the difficulties so far mentioned may be solved by improved techniques and apparatus. There are, however, certain criticisms which have been made of the older sensory psychology which may be leveled against a good deal of the more modern work. In their endeavor to work with controlled, measured stimuli, sensory psychologists tended to limit their study to local effects. Visual adaptation has been studied as if only what occurred in the fovea was of importance, and conclusions have been drawn from studies of foveal adaptation with dark surround as if they were valid for all surrounds. Thus the number of discriminable steps in brightness from zero to high values has been determined by means of split fields against a dark surround. But as Evans has pointed out: "As soon as light is permitted to be present around the split field and the experiment is repeated, radically different results are obtained. . . . In practice in a lighted room the range of intensities over which brightness differences may be seen is very much less than is indicated by the curve obtained by successive adjustments of a two-part field surrounded by black. Furthermore, the range from black up to the surrounding intensity level is very short at low intensity levels and gets progressively *greater* with increasing intensities up to levels at which the eye is blinded" (Evans, 1948, 104-5). It is obvious that the total field influences the measurement of dimensions and, to meet the criticism of field theorists, a wider range of conditions must be incorporated into sensory studies.

That dimensional phenomena can be studied as a function of field conditions is shown in a study of the achromatic point as a function of chromatic adaptation (Helson and Michels, 1948). Observers were required to adjust three primaries of a colorimeter to yield white, gray or black in foveal vision while the rest of the eye was flooded with strongly chromatic light. In this way it was possible to determine the stimulus for foveal achromaticity when the eye was adapted to red, green, yellow, or blue. The locus of neutral points on the color mixture diagram was determined as a function of the brightness of the foveal stimulus relative to that of the surround. It was found that when the foveal stimulus is at least ten times as bright as the surround, the neutral point is independent of the surround, being the same for all chromatic surrounds as for a

dark background. As the foveal stimulus decreases in intensity relative to that of the surround, the neutral point moves toward the surround point on the color mixture diagram, approaching it very closely when the foveal stimulus is about one-tenth as bright (cf. Fig. 8.1). This means that field effects are of negligible importance

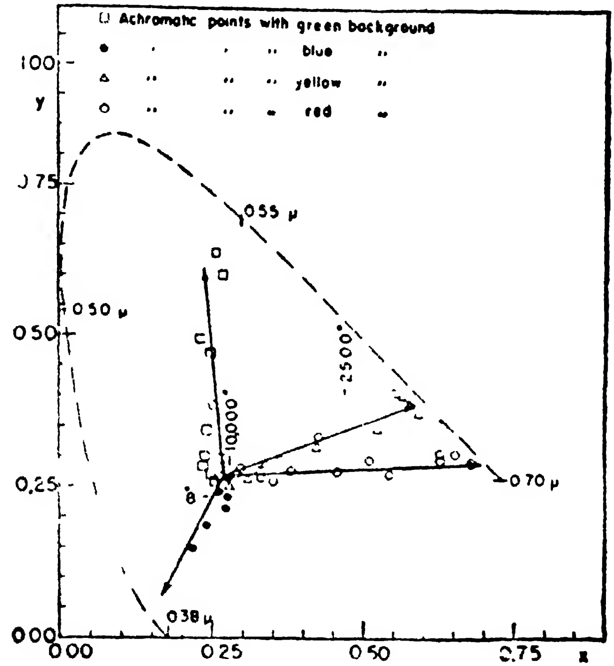

Fig. 8.1. Achromatic loci. The dashed curve shows the spectral locus, with wavelengths in microns; the dotted curve is the locus of black-body radiators, with temperatures in degrees Kelvin; the solid lines join the "white point" with the background points. (From H. Helson and W. C. Michels, *J. O. S. A.*, 1948, 38, 1025-1032, by permission of the authors and the Editor of the Journal of the Optical Society of America.)

so long as the foveal stimulus is at least ten times the background intensity, but they become increasingly important as the ratio changes in favor of the surround. A study by MacAdam confirmed these findings under different conditions of stimulation (1949). Both studies indicate that the state of adaptation and hence the total field, must be taken into account if we are to deal adequately with the dimensional aspects of perception.

Study of field conditions not only broadens the base on which predictions of sensory effects can be made but also makes possible inclusion of new dimensions in perception. This is brought out most clearly when we compare the attributes of colors viewed in small apertures against neutral surrounds with colors seen as surfaces or space-filling objects. As Katz long ago showed (1911), aperture or film colors can be completely described in terms of the classical dimensions of hue, brightness, and saturation. But when viewed as surfaces, achromatic colors cannot be arranged along a single, brightness continuum, for a near "white" matched to a far "white" differs from it in pronouncedness (*Ausgeprägtheit*) and in impressiveness (*Eindringlichkeit*). These additional dimensions solved the paradox arising from the classical view when it was found that *a white surface could be dark and a black surface could be bright*, while retaining white and black character respectively. The two new dimensions behave as lawfully as the older ones. Whites are more pronounced and more impressive the more light they send to the eyes, whereas blacks are more pronounced the less light they send to the eyes, although their impressiveness is increased if they send relatively more light Similarly the chromatic colors may gain or lose in pronouncedness and impressiveness while retaining their hue characteristics (Katz *op. cit.*).

If we adopt as a criterion of a dimension that it must be capable of measurement, then the number of attributes of colors is found to be much greater than the classical three. With the addition of pronouncedness and impressiveness to brightness, hue, and saturation, we already have five (cf. Table 8.1) That size affects hue and hue affects size there can be no doubt. A large red area appears yellowish when compared with an identical small patch, this may be the Bezold-Brucke effect with size serving for intensity. Conversely, hue affects perceived size: blue areas appear larger than equal red stimuli. This effect is so pronounced that in order to equalize the apparent areas in the French tri-color a commission appointed to study the question recommended that the widths of the red, white, and blue should be in the ratios 37:33:30. Hues differ in their apparent distance from the eye. Green and blue have been called retreating colors, red and yellow advancing To make a stage appear

deeper, darker colors are employed for backdrops, preferably blue; hence colors differ with respect to volume or tri-dimensionality. Closely allied to location and volume (which may prove to be a single attribute after all) are the dimensions of warmth and hardness. Reds and yellows are warm while greens and blues are cold, not in the sense of temperature, but as regards feeling-tone, as any painter or stage designer knows. Warm colors lose their pronouncedness in dim illumination to a greater extent than do the cold colors, which may be one of the reasons for the use of the latter in scenes depicting moonlight. That there is a hard-soft attribute of colors was shown by Koffka and Harrower (1931), who found that visual acuity was best with hard figures on hard ground (red, white, and yellow being hard, versus green, black, and blue) next best was hard figure on soft ground, and least good were soft figure on hard ground and soft figure on soft ground. The reason for the superiority of hard colors was found in the tendency of green and blue to overflow their boundaries to a greater extent than do the hard colors.

TABLE 8.1.—PROPOSED DIMENSIONS OF COLORS

Dimension	Chief Determinants and Characteristics
1. Hue	Wavelength, color quality
2. Brightness	Luminous flux, bright to dim quality
3. Saturation	Degree of difference from achromaticity
4. Pronouncedness	White or black quality; chromaticness
5. Impressiveness	Ratio of light from object to total field flux
6. Size	Area, green and blue are larger than red and yellow
7. Location	Red and yellow advance, green and blue recede
8. Volume	Short wave, low intensity colors recede giving depth
9. Hardness	Red white and yellow are hard; green, blue and black are soft
10. Warmth	Red and yellow are warm, green and blue are cool
11. Gloss	Ratio of specular to diffuse reflection of surfaces, also dependent on contrast
12. Transparency	Related to clear-turbid property or hiding power
13. Texture	Micro structure; smoothness of surface
14. Luster	?
15. Sparkle	?
16. Flicker	Temporal change in brightness
17. Shape	A spatial attribute that may affect color
18. Affective Tone	Pleasantness, unpleasantness

Among the candidates for status as new dimensions only the warm-cold attribute has not yet been measured experimentally. Although not recognized as dimensions, gloss and transparency have been subjected to measurement by means of special instruments devised for the purpose--glossmeters and opacimeters These attributes like many of the others range from zero to maximal values. Objects may be glossy or matt, a perfectly matt surface having zero gloss. Hunter (1935) has distinguished five different kinds of gloss with appropriate measures of each Similarly, objects may range along a transparent-opaque continuum Considerable work is necessary by psychologists if these attributes are to be known as well as the classical dimensions Closely related to gloss and transparency is texture which may or may not prove to be a dimension in its own right. In dealing with gloss and transparency, Hunter found it necessary to bring in considerations of texture, as indeed Katz did, when he classified them as separate modes of color appearances Also among the doubtful dimensions we must put luster even though there is no doubt as to the conditions governing its appearance

So far our discussion of the dimensions of color has not been concerned with possible effects of time Colors are, however, affected by temporal factors as shown by such words as sparkle, flicker, glow, and blaze Many of the most dynamic aspects of colors are associated with the ways in which they change in time and space Thus Katz distinguishes between object or quality flicker and illumination flicker. In the former an object seems to flicker and in the latter the flicker is localized in front of objects (Katz, 1911) In view of the many studies based on flicker and phenomena of movement it is safe to say that clarification of the temporal, changing attributes of color may have important theoretical consequences (cf Bartley, 1941).

In proposing shape as a candidate for dimensional status in color we run counter to commonly accepted definitions of color which explicitly identify it with the nonspatial attributes of vision. In view of the fact that all colors have shape and are affected by spatial factors it may be profitable to broaden the definition of color to include spatial attributes. It is becoming increasingly appreciated that the geometry of viewing has important consequences in color

perception, e.g., for gloss and texture. During the war when problems of camouflage were pressing it was recognized that geometry and color are not as unrelated as classical treatments of color phenomena would imply when they omit reference to the role of spatial factors in determining color qualities. Similarly, the affective value of colors has been treated as a problem in feeling and emotion rather than as a problem in perception of color as such. Colors are seldom, if ever, experienced as indifferent or neutral. Colors are either pleasant or unpleasant, acceptable or unacceptable attributes of almost all objects in daily use and in artistic creations. We must therefore seriously consider affective tone as a dimension of color, especially since it has been shown to be capable of quantitative determination (Guilford, 1931).

In this discussion of dimensions we have attempted to show that sensory psychology can meet the charge of reductionism and atomism by studying attributes of perception as functions of field conditions rather than as results of local stimulation. The criticism that sensory psychology contributes little to an understanding of everyday life can be met by showing that knowledge of dimensions, based upon careful measurement, has practical as well as theoretical uses. By enlarging the list of dimensions a greater number of everyday phenomena can be accounted for scientifically. When it is recalled that such great generalizations as the Weber and the Weber-Fechner laws, the Purkinje and the Bezold-Brucke effects, and the Young-Helmholtz and the Hering theories, to mention only a few having to do with the topic of this chapter, were based upon considerations of sensory psychology, there can be no doubt as to the fruitfulness of further research in this field of psychology.

Theory of Objects and the Phenomenological Approach

The account of perception in terms of elementary sensory processes met with resistance from many quarters. Practically all "schools" of psychology arose originally as protests against structuralism. Analysis had been fruitful in chemistry and biology where it had yielded the atomic theory in the one and the cell theory of plants and animals in the other. No wonder it seemed to be the

proper method of attack for the infant science of psychology. Unfortunately the way it was carried over into psychology the method of analysis seemed to strip experience of meanings, values, and even of everyday objects. Wundt recognized this fact when he formulated the law of creative resultants which admitted that a whole is more than a sum of parts, but nevertheless the demand came from all sides for an approach that would do justice to the immediately given in all its richness. The most fertile source of the opposition was undoubtedly the *Akt* psychology of Brentano, because from it developed the theory of objects of Meinong, the phenomenology of Husserl and of Stumpf, and eventually Gestalt psychology.

For Brentano (1874) psychology is the study of mental acts, not contents. Perception is included under judging, the other two acts being loving-hating and thinking. Every act intends an object which in turn exists in the act (doctrine of intentional inexistence). Hence every act transcends itself and the moment of its being. Objects find their significance in the psychic act which calls them forth and the elusive quality of meaning is captured in the doctrine of intention and transcendence. All of this is quite metaphysical, and if Meinong, Husserl, and Stumpf, and some of their students who went on to become experimental psychologists had not been influenced by this doctrine, it would not have much interest for us. The status of objects is not clear in Brentano's system as only the acts are subject-matter for psychology. (Physics does not deal with everyday objects either, according to Brentano.) The effect of this doctrine was to turn attention away from introspective analysis of perception to the study of objects and their properties.

Two main approaches to the study of objects developed. The first, fathered by Meinong, culminated in the work of the Graz school. According to Meinong, objects differ in complexity and may be ordered in hierarchies. Objects of higher order (*Superiora*) require objects of lower order (*Inferiora*) as fundamenta. Thus a square requires lines and angles as founding objects, and colors, which at first sight seem simple, require extension. Relations are also objects of higher order because they require terms which are related (Meinong, 1904). Benussi, a student of Meinong, made much of the fact that a given collection of elements, such as the four

dots in Fig. 8.2, may be combined in a variety of ways to produce many configurations. The dots may be combined to form a rectangle, two vertical lines, two horizontal lines, triangles with an outside dot, oblique lines, etc. A variety of configurations may be formed from a group of elements, and this fact Benussi referred to as configurational ambiguity (*Gestaltmehrdeutigkeit*). To explain lability in *Gestalt* Benussi asserted that configurations are produced by

FIG. 8.2 An ambiguous stimulus which may be perceived in a variety of forms

supra-sensory acts (*aussersinnlicher Produktionsvorgang*) and are therefore mental or ideal objects. Objects mediated wholly by sense, on the other hand, are real. But mental objects are dependent on the existence of real objects as *Inferiora*. While configurations are always ideal sensory data may either truly represent real objects or they may give rise to illusions. This raises the interesting question as to the differences between illusory sensory processes and *Gestalten* which were summarized as follows by Benussi (1904):

Illusory Sensory Processes	*Configurational Processes*
1. Cannot be influenced by observer	1. Can be changed voluntarily
2. Give rise to only one effect (Gray on blue appears yellow)	2. May give rise to different forms from same stimuli
3. Have no limit so long as stimulus is controlled (By making the blue more saturated the gray becomes yellower)	3. May be strengthened or weakened by attitude contra stimulus conditions
4. Are not diminished by exercise, repetition often may strengthen them	4. May be changed by exercise

The division into higher and lower activities, the implication that the organism is passive in receiving sensory impressions, the assumption that sensory processes are more firmly bound to stimuli than configurations are, and the assertion that higher psychic activi-

ties are necessary to account for configurational phenomena were all sharply attacked by Koffka (1919). Koffka argued that far from being products of higher activities, configurations are primary in experience. Sensations, no less than Gestalten, are subject to changes in attitudes and "inner factors." Configurations are as firmly rooted in the physical world and are as predictable as sensory processes, said Koffka. While granting the validity of much of Koffka's criticism of Benussi's position, it should be remembered that Benussi and other members of the Graz school, particularly Witasek (1910), stressed some important aspects of perception which still need investigation.

In emphasizing the fact that a given set of units may give rise to various configurations Benussi touched upon one of the most difficult and interesting problems in perception. Why does the same group of "elements" arouse different configurations in different observers or in the same observer at different times? The appeal to attitude hardly helps in understanding the problem. That it merited further investigation is now borne out by studies of perception having to do with the effect of needs and other inner factors on perceiving, and also by the possibility that, as Frenkel-Brunswik has pointed out, tolerance or intolerance for perceptual ambiguity may be symptomatic of other personality traits. As she has so well put it: "If such intolerance should turn out to be a formal characteristic of the organism independent of content, experiments on perceptual ambiguity could be used as diagnostic tools" (Frenkel-Brunswik, 1949, p. 126).

A preliminary experiment along these lines cited by Frenkel-Brunswik contains such interesting possibilities for research into the relation of perception to personality traits that her brief statement of it bears direct quotation:

> ". . . first the picture of a dog was shown, followed by a number of pictures representing transitional stages leading finally to the picture of a cat. At every stage the subjects were asked to identify the object on the given card. In spite of the fact that the cards were not too well drawn for the purpose, distinct trends became evident. The prejudiced group tended to hold on longer to the first object and respond more slowly to the changing

stimuli. There was greater reluctance to give up the original object about which one had felt relatively certain and a tendency not to see what did not harmonize with the first set as well as a shying away from transitional solutions. Once this perseveration was broken, there seemed to be in this group either a spell of haphazard guessing or a blocking by the uncertainties inherent in the situation. It may well turn out upon further evidence that intolerance of perceptual ambiguity is related to a broader psychological disturbance of which prejudice—itself often a deviation from the prevalent code, especially in school—is but another manifestation." (Frenkel-Brunswik, 1949, p. 128.)

Benussi himself saw the possibilities of relating perceptual functions to personality traits. In experiments having to do with determination of what he referred to as "gestalt time" (*Gestaltzeit*), he found subjects who typically saw the Müller-Lyer illusion best in short exposures (82-100ms) and another group who did not get it unless the exposure times were comparatively long (1000-1600ms). The first group saw the parts of the figure while the second group saw the whole figure best when the time was long. One group tends to react to parts first, requiring time to perceive the whole, the other group tends to perceive the whole first and requires time to isolate the parts. Either attitude, for Benussi, was natural, although the Wertheimer configurationists were later to maintain that only perception of wholes is natural. Benussi suggested that his two groups represented two different types of individuals: an analytic and a synthetic type (Benussi, 1914), but he did not pursue the matter further. Interest in personality had not then developed to the point where psychologists could follow up the full implications of such findings.

We have previously indicated that Brentano's *Akt* psychology was the starting point or inspiration for Meinong's theory of objects (*Gegenstandstheorie*) and therefore for much of the theoretical framework in which the members of the Graz school interpreted their experimental results. Unfortunately the weakness of their theoretical position prevented full appreciation of many fine experimental contributions (cf. the list of Benussi's publications on gestalt phenomena given by Helson, 1926). The division of mental activities into sensory and higher mental processes entailing the

further distinction between real and illusory sensory data and ideal configurations was laden with philosophical difficulties and was too awkward to furnish a satisfactory basis for interpreting many of the phenomena of perception. A simpler approach was needed and this, the second offshoot of *Akt* psychology, was furnished by the phenomenology of Husserl. It paved the way for the incisive, radical stand later taken by the configurationists, furnishing the basis for their nativism, reliance on intuition, and their emphasis on whole-properties.

> As advocated by Husserl phenomenology rejects such distinctions as inner-outer, real-unreal, objective-subjective, in their usual meanings. Immediately intuited experiences are taken at their face value without inquiring into their origins. Experiences are regarded as self-contained wholes. Each perception carries immediate certainty about something which Husserl called its essence. Higher mental processes are not necessary to explain meanings, values and the objects of thought. Experience in this sense exemplifies, it does not validate since validation refers to other experiences which in turn exemplify some essence. In perceiving a tree we might reflect "There is a tree, existing in real space and time, and here am I, a real observer, perhaps having an Ego, looking at the tree." But phenomenologically there can be no question as to whether the tree is real or imaginary, whether I am of the same stuff as the tree, or if real relations exist between the tree and myself. If the experience carries within itself marks of reality, i.e., what is meant by reality, then it can be said the tree is real. Experience is thus self validating (Husserl, 1913).

The phenomenological method, with its reliance on immediate experience, represents an emphasis on perception as it is naively given. Phenomenology attempts to rid itself of all assumptions, deductions, and "errors" of logical or inductive processes. Analytical introspection could not, in its view, be a valid approach to perception because it would create experiences different from the original. The structure or organization of perception must be taken as it is found, and hence it becomes a postulate. While some adherents of the phenomenological approach rejected all causal-genetic investigations of stimulus conditions and objected to neurological hypotheses (e.g.,

Linke, 1919), the configurational psychologists emphasized the larger context in which perceptual processes arise, particularly neurophysiological correlates. The rejection of psychological processes not immediately observable, such as the memorial and imaginal processes often invoked to explain illusions, shows the influence of phenomenology on gestalt psychology.

In closing this discussion of phenomenology it should be pointed out that its influence has extended far beyond the confines of perceptual theory, albeit by way of it. Descriptions of learning and problem-solving in terms of molar as against molecular concepts must be regarded as a carry-over of phenomenology to the field of behavior. Certain aspects of nondirective or client-centered counseling can be understood better when viewed as an application of phenomenology to problems of therapy. Emphasis on the role of the counselor as merely an aid in helping the patient to clarify his own outlook and exclusion of interpretation by the counselor of the patient's reactions (at least during the interview) point up certain essentials of the phenomenological approach as applied to psychotherapy (cf. Combs, 1949).

The Configurational Approach

In order to understand the present, some knowledge of the past is necessary. This truism would hardly need repetition were it not for the tendency to regard theories as having sprung full-grown from the head of some innovator much as Minerva is reputed to have done from the head of Zeus. We have already examined two theories having to do with perception of objects, *Gegenstandstheorie* and phenomenology, both of which were important. Still other developments were to play a part in the new psychology of perception, *Gestalt* psychology. One of the most fertile sources of future developments was Ernst Mach's book, *Analysis of Sensations*, written in 1886. In it Mach showed how mere changes in spatial orientation, like turning a square from its side to a corner, and changes in temporal intervals could bring about radical changes in visual and auditory configurations. The problem of super-summative properties of wholes engaged von Ehrenfels (1890) who gave the name

"form-quality" to any property of a complex of elements arising as a result of their togetherness. By including such temporal phenomena as reddening, blanching, and "growing bluer" (!), von Ehrenfels extended Mach's concept of space- and time-forms to all configurations having a definite direction. Schumann (1900-04) was not satisfied to attribute form-qualities merely to form and proceeded to investigate concretely how various factors influence configurations. Thus he showed, using many illustrations, that equal distances between elements contribute to group formation; nearness of elements favors group formation; contours are important in delineating objects, incomplete figures tend to be seen as complete, and ambiguous figures tend to be perceived as "good figures."

Interest in organized wholes grew apace, although there was considerable controversy as to how the data should be interpreted theoretically. No unifying theory of the many phenomena emerged however, until Wertheimer suggested that a new approach was necessary. Addition of elements, even new ones such as the form qualities of von Ehrenfels, could not explain whole-properties. Wertheimer declared (1912) *Gestalten* are *sui generis* and require the assumption of whole-properties as basic postulates. Wholes are not built out of parts, rather, parts derive their properties from the wholes in which they are imbedded. Wholes are no less immediate than their parts and may precede them, e.g., when we recognize a melody before we do its mode or the individual pitches. Sensory attributes are products of analysis, not natural units of synthesis. Stimulus correlations should be in terms of properties associated with extended, whole objects which immediately greet the eye or the ear and are described in such terms as roundness, pointedness, rugged, harmonious, and the like.

The psychological lexicon was thus broadened to include phenomenological properties. Meanings, values, even "futurities," may be immediately apprehended and require no special processes for their arousal. Affective and aesthetic qualities are as much functions of organization as their form or color. Objects have uses, they satisfy and annoy, and are intimately bound up with emotional drives which may determine the way they are perceived. Hence physiognomic character (Koffka, 1935) and demand character (Lewin, 1926)

must be regarded as intrinsic properties of perception. Here the influence of phenomenology makes itself evident: but, if it consisted merely in bringing phenomenology into psychology, configurationism would have had much less impact on contemporary theory than was the case. Besides making the concept of *Gestalt* central, with all that it implied, the configurationists also succeeded in showing that whole concepts in psychology require whole concepts in physiology and physics. This may prove to be their most important contribution and merits discussion

The demand for total physiological processes to go with perceived configurations was voiced in Wertheimer's original paper wherein the argument is somewhat as follows When an observer perceives movement from stationary stimuli exposed in rapid succession (the phi-phenomenon), it is impossible to analyze the perception into something stationary plus images of movement supplied by memory or imagination Phenomenologically, if the phi-phenomenon cannot be distinguished from a moving stimulus it must be regarded as identical with actual motion At this point Wertheimer went a step farther than the strict phenomenologists and postulated physiological processes having whole properties to account for the perceived movement. The brain processes for phi must be essentially the same as for "real" motion, if the two kinds of movement are phenomenologically identical

The importance of stressing adequate physiological bases for all perceptions, whether "illusory" or real, cannot be overestimated. It led to the theory of physical *Gestalten*, thereby obviating the charge that configurationism was only another brand of subjective psychology. Perceptual properties were henceforth derived from laws governing physical systems Thus the seeing of a broken circle as a complete circle, when exposed tachistoscopically, was ascribed to the tendency of simple physical systems to come to equilibrium in characteristic manners, much as a soap bubble assumes spherical form to enclose the largest volume within the smallest surface area. Principles of simplicity, minimum expenditure of energy symmetry, and completeness were shown to be characteristic of both physical and psychological configurations (Kohler, 1920)

The transition from physical to psychological systems has been made most explicit in the principle of isomorphism. According to this psychophysical postulate, "Experienced order in space is always structurally identical with a functional order in the distribution of underlying brain processes" (Köhler, 1947, p. 61). A similar assumption is also made to account for the configurational properties of temporally extended events such as melodies. The correspondence is assumed to be between the *brain process* and *perceived object*, because perception may reproduce the external objects more truly than the energies from stimuli (light or sound waves) do. The assumption of isomorphism is an attempt to answer a number of questions which have plagued every scientific system of psychology. They come to the fore especially in the theory of *Gestalt* which rejects the analytical method as a means of solving many difficulties By identifying the formal, ordered aspects of perception with neural processes it becomes unnecessary, as the Graz school had done, to postulate higher mental activities to account for organized properties of wholes The traditional gap between mind and body is bridged by showing there exist physical systems which possess properties as wholes not to be found in their parts The division into higher and lower, primary and secondary, objective and subjective properties seems to be no longer necessary if patterning and its concomitant effects are granted for physical as well as psychological systems. Behavior, which Watson and his followers had insisted was the only objective, legitimate study for psychology, becomes as much a reflection of psychological events as these are of neural activities. Configurational psychology has thus been able to achieve unity not only at the psychological level but also within a larger psychophysical context embracing both psychological and behavioral processes

The configurational approach to perception succeeded in bursting the bonds of a too-narrowly conceived existentialism which limited studies of sensory processes to analytic introspection of the results of local stimulation. At first the platform of the configurationists demanded banishment of all unobservable processes and entities from psychology. It was believed that contemporary field properties would be sufficient for configurational phenomena. (Cf. the summary of *Gestalt* psychology up to 1926 by Helson, 1925; 1926.) Some phe-

nomena, however, seem to be determined outside the behavioral field. By 1935 we find Koffka saying that a number of effects (such as recognition, unconsciously motivated acts and skills) "fall outside the present behavioural environment" (Koffka, 1935, p. 51) and require the assumption of forces operating in the nervous system.

The appeal to configurational processes in the nervous system to serve as neural correlates of phenomenal fields may be taken as an indication that configurationists feel the need for explanatory as well as descriptive principles. But physiological fields are essentially the same as the phenomenal fields they are designed to explain since the former are endowed with the same properties as the latter. The immediately given, whether in phenomenal or neural terms, does not provide the correlations necessary for understanding itself. It is usual in scientific theorizing to postulate a different set of entities and relations from the ones observed in order to explain them. As a concrete example, consider two yellow illuminants which are a perfect visual match. Phenomenologically they are identical. Objects viewed in the two may, however, have very different colors, if one is a spectrally pure yellow and the other is a metamer composed of spectral red and green. To predict the colors of objects in the two illuminants it is necessary to know their spectral composition. We cannot dispense with causal-genetic analysis for underneath phenotypical similarities may be genotypical differences. To complete the causal picture, stimulus-perception correspondences must supplement central neural isomorphisms. We must look to the external world and its control as well as to the central nervous system if we are to understand and predict behavioral processes.

It may be argued that stimulus correlation with behavioral data may be changed by factors within the organism. Hence, the argument would run, it becomes necessary to develop a theory of the contribution made by the organism, particularly of what is contributed from its previous history. In configurational theory the trace has served this function. Let us examine it briefly. Every act, every experience leaves a trace in the central nervous system within a given region which we may label A. This residual is to be regarded as a dynamic field of force or tension system. Stimuli set up processes in the present which affect areas which we may designate by B. If

the past is to have any effect on the present it must do so through the communication or assimilation of region A with region B Recall, reproduction, recognition, and the influence of memory generally can thus be considered to be the product, AB, of present processes and tension systems left over from past experience What other psychologists have attributed to association, to memory images, to needs, and to "autistic" factors, the configurationists tend to explain by traces. Individual differences in response to ambiguous figures, and the many ways in which stimuli may be perceived, judged, remembered or imagined, have been interpreted by properties ascribed to trace systems These properties vary from problem to problem in accordance with Koffka's admission that "we construct our physiological field in accordance with, and directed by, the observed properties of" the behavioural environment (1935, p 63)

While the doctrine of traces has served to correct some of the loose thinking about the effects of past experience, by concretizing its possibilities in physiological terms, it fails to provide the quantitative basis that the phenomenological approach so badly needs Traces are still as qualitative as phenomenal fields Trace fields have so far furnished only qualitative principles where quantitative predictions are necessary to decide between opposing theories That residuals from past experience can be quantitatively evaluated has been shown in a number of recent studies (Michels and Helson, 1949, Johnson, 1949) Applying an equation embodying one postulate of adaptation-level theory that the neutral point in judging a series of stimuli with respect to some dimension is a weighted mean of series, background stimuli, and residuals or traces from past experience, Johnson (1949) evaluated the effects of various amounts of practice in judging low (high) pitches on succeeding judgments of high (low) pitches As the number of practice trials with the first series increased from 1 to 5, the weighting factor denoting its relative influence on the second series increased from 0.3 to 1.22. Thus traces were quantitatively evaluated The implications of this approach for learning theory are pointed out by Johnson who shows that even though we are dealing here with the effects of repetition, the equations employed describe an "integration, generalization, or

patterning of practice effects" (*op. cit.*, p. 858), since the theory is concerned with the center of a judgment *scale*.

One final criticism of the configurational theory raises a paradox. Although the configurationists regard the phenomenal properties of fields as basic data they assert with the strict dimensionalists that size, shape, and color constancy cannot be accounted for by sensory processes. Thus Boring asserts that constancy "depends upon an integrative property of the brain and is not a function of sense organs at all" (1946, p. 99). Koffka and Kohler (*opp. citt.*) attribute constancy to "organization." Yet it was shown by Bornemeier that lightness constancy can be predicted from the sensory dimensions of the visual field, provided the adaptation-level of the eye is taken into account (quoted in Helson, 1943). Judd (1940) also showed that hue and chroma can be similarly predicted. Neither the theory of brain integration nor the concept of organization makes possible concrete prediction of what will be seen under specific conditions of observation. Constancy is at best only approximate and depends upon many factors in the field as well as upon residuals from past experience. We do not see white papers in shadow exactly the same as in full illumination nor do we see round objects turned out of the frontal parallel plane as perfectly round. We need a theory that enables us to state what the approximation to constancy will be under given conditions, what will occur when the limits of tolerance for changes in stimulus conditions have been reached, and how the field will appear when objects can no longer be said to look "normal." It is obvious that a quantitative theory of configurational effects is now necessary in order to move from the plane of description to that of prediction.

Reinforcement, Sensory Organization and Stimulus Generalization

In discussing the memory trace and its influence on perception we are led naturally to a consideration of problems common to learning theory and to perception theory. While perception theorists have been forced to take account of possible effects of learning on perceptual processes and have been eager to apply their principles to

learning, workers in the field of learning have concerned themselves hardly at all with perceptual theory. It is no exaggeration to say that one may read whole articles and even books on learning without becoming aware that animal subjects are endowed with sense organs and need them in order to solve problems of maze, Skinner box, and problem cage. There are, however, signs of rapprochement. Recent concern with the continuity-discontinuity problem and the role of sensory organization and reinforcement in learning involve questions that relate to the nature of perception as much as to the nature of learning.

Woodworth has pointed out with inescapable logic that reinforcement must operate to change perception if any learning, even conditioning, is to occur. In answer to the question: "What is reinforced in conditioning?" Woodworth answers that it cannot be the conditioned-reflex because it hasn't appeared nor can it be the conditioned-stimulus conditioned-response connection because it isn't formed! It must therefore be something in the organism which is reinforced from the first trial, and it must belong to the receptive and not to the effective part of the organism's total behavior. When alternatives are to be discriminated the distinction to be made is between stimulus objects and not between motor responses. It is the *difference* between stimuli (such as alleys in a maze) that has to be reinforced. Hence perception must be reinforced (Woodworth, 1947). Reinforcement must be effective in perception before it can operate in learning. As Woodworth puts it: "The conditioning experiment is really concerned with the establishment of a new perception. . . ." (1947, p. 124). While this conclusion fits with the point of view of field theorists regarding the nature of learning it does not deny the efficacy of reinforcement which conditioning theorists have stressed, nor does it exclude the possibility that sensory stimuli may interact without reinforcement as Brogden (1947) has shown.

The question must still be faced: "How can reinforcement establish a new perception?" Not content with explanations in terms of brain or field dynamics which allow little or no role to so-called mechanical factors in perception and learning, a number of writers (Maier and Schneirla, 1942) postulate sensory integration in which

"one stimulus (the conditioned stimulus) as a result of contiguous presentation with a second (the unconditioned stimulus) gradually acquires the functional properties of the second" (Birch and Bitterman, 1949, p. 294). In addition, Birch and Bitterman postulate progressive modification of response to given stimulating conditions as a result of reward and punishment. Together these postulates, for which there is considerable experimental evidence, provide for the effects of contiguity and affective factors (as well as reinforcement by other means) in molding behavior

Still another problem in conditioning points toward the field of perception for its solution This is the problem of stimulus generalization in conditioned responses. It concerns transfer and its brain dynamics. Pavlov, as is well known, had postulated irradiation followed by concentration of cortical nervous excitation aroused by stimulation. Irradiation was postulated to account for generalization, concentration for discrimination Both experimental data and the neurological theory of generalization have been subjects of considerable controversy, as pointed out by Razran (1949). Lashley and Wade (1946) maintain that gradient of habit strength is a product of variable stimulus threshold rather than due to a spread of associative processes; generalization does not occur in conditioning to a single stimulus, but is somehow a function of differential training with two or more stimuli along the same dimensional continuum. Furthermore, Lashley and Wade conclude that the dimension is primarily a function of the organism and only secondarily determined by the physical properties of the stimuli. Going a step further, Razran formulates a theory of categorizing or rating-scale behavior which may serve to integrate basic facts in conditioning-learning and facts in recent studies of perception. According to this writer:

"The doctrine of a qualitative, categorizing, 'rating scale' type of CR gradient . . . is based upon a statistical and logical analysis of total evidence, which . . . demonstrates that there is a true CR gradient, but this gradient is very qualitative and very crude, consisting of only a few steps, perhaps more steps in human beings than in dogs, but few just the same. Apparently, when human beings or dogs that have been conditioned to

> some stimulus or object are confronted with some new non-conditioned but in some way related stimulus or object, they categorize or rate the new stimulus on some sort of crude similar-dissimilarity scale. With human subjects, introspections actually reveal such categorizing attitudes as 'similar,' 'very similar,' 'not so similar,' 'somewhat similar,' 'dissimilar,' 'very dissimilar,' and the like-attitudes that apparently control or even initiate the generalization responses. And some such categorizing behavior is operative also in animals.
>
> "This categorizing (is) very dynamic and changeable *and varying much more with the organic dimensions of the organism than with the physical dimensions of the external stimuli in the CR situation*" (Razran, 1949, p. 362).

It is evident from this quotation, as well as from the preceding discussion of sensory integration, reinforcement and gradient of habit strength, that learning theorists have come to conclusions essentially similar to conclusions reached by perception theorists who have dealt with the problem of frames of reference and the structure of perceptual fields as functions of the adaptation-level. Both types of theory are causal-genetic and agree in recognizing the importance of frequency, contiguity, area, intensity and the many other measureable aspects of stimuli in determining behavior. Thus whether a weight will feel light or heavy, a gray will be light or dark, or a taste will be pleasant or unpleasant will depend upon the number of times it is presented, the other stimuli simultaneously present, and on residuals from past experience as well as upon its physical properties. Perceptual properties may be intrinsic in some sense, but they are nevertheless subject to influences which modify the state of the organism. If the determining factors are such as to result in progressive modification of the perception with resultant changes in behavior, learning is said to take place. Only because perceptual properties have been regarded as immediately given or ready-made has it been possible to regard perception as free from learning effects. On the other hand, it must be recognized that learning does not occur without awareness, although at subliminal levels sometimes. It would therefore seem to be a necessary conclusion that learning theory can no more neglect factors influencing perception than perception theory can neglect factors influencing learning. It is thus

possible, within the framework of causal-genetic theories of perception and of learning, to take account of factors operative in both. We shall return to this problem in the concluding section of the chapter.

Needs, Motivation and Perception

Throughout the history of psychology it has been recognized that inner as well as outer factors determine behavior. Recently, inner factors such as hunger, likes and dislikes, frustrations, and personal values have been investigated in their effect on perception and related activities. As Murphy has put it: ". . . wherever our needs differ we literally see differently. Much of the process of individual perception depends upon the force of *past* wants, the person's need to disentangle and restructure in terms of the situations with which he has had to cope . . . we have *learned* to perceive as we do, and needs play just as important a role in guiding the structure of our motor habits" (Murphy, 1947, 338f.)

Since there is no doubt that perception is to some extent determined by the nature of the stimulus and receptor, it is necessary to define the sphere of influence of these inner factors. This has been done by emphasizing the difference between structural or autochthonous factors, which are the old Titchenerian "common core" of perceptions, and functional or behavioral factors, which are due to individual differences in needs, motives, expectancies and past experiences. The more ambiguous the structural determinants the more behavioral factors are presumably operative. Again we find *Gestaltmehrdeutigkeit* a challenging field for further investigation. Many types of experiments have been performed to show how inner factors can swing perception one way or another. We shall restrict discussion to a few high points of this interesting and fruitful appproach.

Typical experiments illustrating the influence of needs on perception are found in the following examples: subjects viewing ambiguous figures were rewarded or punished as they reported one or another phase with the result that the rewarded phases were perceived more often than the punished ones (Schafer and Murphy,

1943); poor children were found to overestimate the size of coins to a greater extent than a wealthy group, presumably because of the greater need for money among the poor (Bruner and Goodman, 1947); subjects overestimated dollar sign and swastika as compared with neutral symbols (square with two diagonals) ostensibly because of their strong emotional connotations. Yet the dollar sign is supposedly positive and the swastika negative so that underestimation might have been expected in the case of the latter (Bruner, 1948). These and many more experiments have been cited to show the effects of inner factors on perception but their interpretation is beset with many difficulties.

Murphy has pointed out that need-motivated theories of perception have not yet been formulated to yield predictions for concrete cases (1949, p. 54) Sometimes workers are not able to duplicate a given set of findings as when Carter and Schooler (1949) failed to vertify the Bruner and Goodman results just quoted Pastore in reviewing most of the experiments done to date (1949) in this field finds basis for criticizing the experimental design, statistical treatment, and interpretations of the data

What at first sight seems to be an almost self-evident proposition—namely, that internal factors influence perception—proves to be laden with systematic difficulties Consider the experiment by Postman, Bruner and McGinnies (1948). The personal values of 25 subjects were measured by the Allport-Vernon test. The subjects were then tested for recognition of six sets of words belonging to the values previously measured The words were exposed tachistoscopically at increasing times until they were correctly perceived. The hypothesis was that recognition times would be lower for words related to dominant interests than for lesser ones The results, which were statistically reliable, showed that "the higher the value represented by a word, the more rapidly it is likely to be recognized" (1948, p. 148). But as Hochberg and Gleitman (1949) point out, the meanings of the words had to be known in order for blockage to occur. How this could happen is a mystery, if it does occur in this way.

Another experiment requires not one but several need hypotheses to explain the results (Levine, Chein and Murphy, 1942). Subjects

were required to verbalize an association aroused by looking at ambiguous figures exposed on a screen after one, three, six, and nine hours of food deprivation. The results showed some relation between intensity of hunger and food associations but not in the simple way expected. The number of food responses to achromatic cards increased at three- and six-hour intervals but decreased at nine hours, whereas with chromatic stimuli they increased to three hours and thereafter decreased. The authors postulate one need, hunger, to explain the increase in food responses and a need to leave the field to explain the decrease in food responses at longer intervals.

The experiments dealing with needs, attitudes, defenses, and the like on perception have so far yielded no satisfactory theoretical framework in which they may be ordered and understood. The concepts have all the richness and ambiguity of undefined, everyday terms. Do we really understand the meaning of such terms as needs, motives, attitudes? Have we the knowledge to control them as variables in the way that Bruner and Postman (1949) have rightly seen that they must be controlled if progress is to be made toward understanding their effect on perceptual behavior? It may be necessary to resort to animal experimentation to settle some of the salient problems in this field. In view of the fact that it has been possible to demonstrate that animals are subject to many of the "illusions" in much the same way that humans are known to be, may it not be possible to study the effects of hunger, thirst, and sex needs on perception in organisms below man? Inner factors, uncomplicated by verbalization, meanings, and memory, could then be investigated as they affect perception. All in all, further investigation of the inner determinants of perception should yield valuable contributions to both fact and theory.

A General Theory of Perception

We have so far discussed some of the main currents in perceptual theory and have found that they may be related to differences in basic approaches to a number of fundamental problems. First, we have seen that the method of introspective analysis has given

way to dimensionalism which continues the earlier interest in sensory attributes, peripheral mechanisms, and correlations of experiential data with stimulus energies. Second, there is configurationism with its emphasis on phenomenological properties of objects and its postulation of brain fields having essentially the same properties as perceived wholes. Third, there is a growing accumulation of evidence from conditioning and learning showing that behavior is modified by stimuli through their effect on perception. And fourth, there is the attempt to show that inner factors may swing the scales in ambiguously structured situations to yield perceptions in keeping with needs, values, and feeling states.

In considering the various approaches to fundamental problems in perception we are struck by the lack of theories having quantative bases or implications. So far as specific aspects of perception are concerned we do have some mathematical theories. General theories of perception tend to be expressed in qualitative or verbal terms which cannot be tested quantitatively. Decision in favor of non-quantitative theories seems to be more a matter of general philosophical presuppositions than of concrete, experimental evidence since deductions from them cannot be stated sufficiently precisely to be settled by an appeal to quantitative data. Hence our first requirement for a theory of perception is that it must be capable of quantitative formulation in order to yield concrete predictions. A theory must be able to predict not only what will occur but also the extent or the degree to which it will occur. Historically the demand for quantification was left to psychophysics and is still so regarded by many psychologists. But when it is considered how much more is known about qualitative phenomena when they are carefully measured we can no longer regard quantification as a psychophysical luxury indulged in for merely academic purposes. Our aim must be to push mathematical theorizing into every aspect of perceptual behavior.

Several other considerations are also of importance in formulating a theory of perception. A theory must account for both local and field effects by a single set of assumptions, if possible. It must take into account automatically operating factors such as repetition, recency, contiguity, intensity, and the like, as well as effects due to

organization or patterning. In general, learning theorists have tended to stress the former which are more susceptible to quantitative treatment, and perception theorists have tended to stress the latter. At present it would seem that the concept of organization is in need of quantitative formulation and the so-called automatic factors operating to modify perception need to be rationally incorporated into a theory. Along with the automatic factors must be reckoned the inner determinants of perceptual processes since investigations of the effects of motivation, needs, and attitudes have so far yielded only correlations with perceptual data without revealing their underlying mechanisms. There is no reason to suppose that internal factors operate in fundamentally different fashion from external stimuli in their effect on perception. Consequently we should expect a single theory to be adequate for both. Finally, a theory of perception must make provision for the contribution of the organism since the state of the organism can profoundly modify the effects of stimulation. This means that perception must be envisaged as a functional process in the business of living or as a factor in the adaptation of the organism to its surroundings.

The theory to be proposed here attempts to meet the demands we have just outlined for a satisfactory approach to perception. One would indeed be foolhardy to assert that any theory can meet all the demands which may be made upon it. In so far as it makes possible some or all of the objects which we have outlined, our theory may be regarded as a tool for exploration, suggesting new approaches to old problems and opening up new areas. Its primary purpose is not so much to displace other theories as to supplement and broaden them. Let us turn to an exposition of its main features by considering the facts upon which it is based, the assumptions made to explain these facts, and some implications which have been verified by experimentation.

The theory of adaptation level starts from the fundamental fact that experience is ordered. In perception objects and events are isolated, identified, related, and ordered within frames of reference which are revealed in categorizing attitudes expressed in such verbal terms as "This is very beautiful," "That is a small building for an armory," or "Parallel lines converge in the distance." The order

revealed in perception cannot be assumed as an *a priori* fact because it is obviously a creation of the organism, differing for different people with the same stimuli and often for the same individual at different times. Even when external stimuli come to us already ordered we could not perceive them as ordered unless internal conditions were favorable to perceive order. All forces impinging upon the organism, as well as all forces from within, are utilized in the creation of order. There can be no ultimate distinction between so-called automatic or blind factors and forces of organization or between inner and outer determinants of behavior, once their mechanisms are understood. A satisfactory theory of order must apply to all conditions under which it appears.

One of the chief characteristics of organic patterning is found in the establishment of neutral, indifferent, or zero regions of functioning in all types of behavior. These neutral regions represent the organism's centering with respect to the stimuli confronting it and are the true zeros of functioning. Thus if a weight is said to be heavy, or a light dim, or a painting beautiful it is because the object in question appears above or below the indifference point of functioning. Such points can be said to represent the adaptation-level of the organism with respect to given stimuli. We find, for example, that the neutral stimulus in a series of weights ranging from 200 to 400 granrs is about 250 grams, while in a series ranging from 400 to 600 grams the neutral stimulus is about 475 grams. If a background or comparison stimulus of 900 grams is introduced, the neutral stimulus becomes 350 grams in the first case and 550 grams in the second case. The adaptation-level, in general, tends to be determined largely by the values of the series and background stimuli. The effects of past experience are usually not sufficient to displace the adaptation-level from within the stimulus continuum. When the eyes are flooded with homogeneous radiation certain stimuli are perceived as achromatic, showing that even under extreme conditions of stimulation the organism maintains its own zero of functioning (Helson and Michels, 1948). The position or value of the functional zero determines the structure of the perceived field. Quantitative evaluation of the adaptation-level and of the relative contributions of series, background, and residual stimuli

in determining it, are main considerations in adaptation-level theory.

How do stimuli and their residuals affect the adaptation-level? We assume that all effects of stimulation, past as well as present, are pooled to form a single level with respect to given classes of stimuli. Evidence for pooling comes from many different sources. In vision and with lifted weights the adaptation-level has been shown to be a function of the series and background stimuli, changes in one or the other bringing about changes in the level (Helson, 1947, 1948; Michels, and Helson, 1949) Similarly judgments of sound intensities have been shown to be functions of series and preceding stimuli (Rogers, 1941) Under some conditions, previously experienced stimuli may have considerable effect on level as shown by the work of Johnson, previously referred to (1949). Certain results with sensory pre-conditioning can only be explained as a result of pooling Thus Brogden (1947) found that a stimulus (light) paired with a sound stimulus which was later, in turn, paired with shock could arouse the conditioned response even though it (the light) had never been directly paired with shock Even stimuli which are not consciously perceived have been shown to exert effects on judgments and to be detectable by galvanic skin responses (McCleary and Lazarus, 1949) Pooling would therefore appear to be possible at purely physiological levels below the psychophysical regions where conscious judgments operate There is even evidence for cross-modality and for sensori-motor ooling as Werner and Wapner have pointed out (1949). Studie dealing with the formation of frames of reference and scales of judgment having social, ethical, affective, and aesthetic reference also point to pooling as the basic factor in the establishment of adaptation-level (Hunt, 1941; Hunt and Volkmann, 1937) Pooling may thus be regarded as a basic characteristic of all organic functioning

The concept of pooling makes possible a quantitative theory of phenomena associated with adaptation-level We assume that the effects of stimulation, past as well as present, affect the adaptation-level in accordance with their frequency, area, intensity, affective value, nearness, and so on. In accordance with this assumption we can say that a suitably weighted average represents the organism's

adaptation to a given set of stimuli. Formulas have been devised for determining the relative weights to be assigned to factors chiefly responsible for establishment of level. In general, three separable factors have been found important: the stimulus attended to or being judged, all other stimuli forming the background stimulus, and effects of previous stimuli forming the residual stimulus. Methods of evaluating the contributions of each of these have been shown to be applicable to various types of stimuli and conditions of presentation (Helson, 1947; 1948; Michels and Helson, 1949; Johnson, 1949). A number of phenomena have been shown to follow from the relation of series and background stimuli to adaptation-level. Thus time-order errors have been shown to arise from the decentered position of the indifference point with symmetrical series stimuli owing to the fact that the adaptation-level tends to be below the arithmetic mean of the stimuli (Helson, 1947). Repulsion effects of background stimuli and of satiated stimuli with the resultant "paradoxical distance effect" have been shown to follow from their effect on adaptation-level (Nash, 1950). Qualitative re-structuring of perceptual fields with corresponding changes in judgments of stimuli have been shown to be due to changes in the adaptation-level whereby positive stimuli (above AL) become neutral or negative (below AL). Finally, effects of social stimulation on individual perception and judgment have been evaluated as a function of size of group, showing that social interactions may pool as well as the effects of physical stimulation (E. Schein, unpublished M.A. Thesis, Stanford University Library, 1949)

Adaptation, as here conceived, is not merely a psychological concept. It represents physiological states as well. It must not be regarded as the static, end-result of steady activity. Rather, the adaptation-level is a moving, changing region, varying in time even in the absence of stimulation since physical processes do not, as a rule, remain stationary in complex systems We are thus led to inquire if a physiological model exists to envisage the type of mechanism implied by the concept of pooling and averaging. Such a model has recently been proposed by Pitts and McCulloch (1947) to account for perception of gestalten, geometric objects, or what they refer to as "invariants." As stated by them the problem is to

produce the same output in nerve nets for every input belonging to a given invariant. Since their invariants prove to be averages and come very close to our concept of adaptation-level, we may provisionally take their model as a possible one for adaptation-level phenomena. For example, let us see how they deal with the concrete problem of seeing "squareness" and how the eyes come to fixate a square. We quote:

> ". . consider the reflex arc from the eyes through the tectum to the oculomotor nuclei and so to the muscles which direct the gaze . . the superior colliculus computes by double integration the lateral and vertical coordinates of the 'center of gravity of the distribution of brightness' referred to the point of fixation as origin, and supplies impulses at a rate proportional to these coordinates to the lateral and vertical eye muscles in such a way that these can turn the visual axis towards the center of gravity. As the center of gravity approaches the origin its ordinate and abscissa diminish, slowing the eyes and finally stopping them when the visual axes point at the center of brightness. This provides invariants of translation. If a square should appear anywhere in the field the eyes turn until it is centered, and what they see is the same, whatever the initial position of the square. This is a reflex mechanism, for it operates on the principle of the servomechanism, or 'negative feedback'" (1947, p. 137).

A mechanism such as Pitts and McCulloch describe may be made to serve not only for centering forms so that their invariant properties are preserved but also to account for the establishment of neutral points within extended spatio-temporal fields. The point of centering, which for us is the adaptation-level, need not, as they also point out, be the physical center of gravity. With a mechanism for averaging, it follows immediately that parts of the field away from the "center" can be regarded as gradients or deviants from the average, as it is envisaged in the theory of adaptation-level. The Pitts and McCulloch model fits into our general conceptual framework although further elaboration is necessary for specific problems.

The theory of adaptation-level accounts not only for the invariants in perception but also for individual differences in perception in the face of the same stimuli. Individual differences may arise from different residual factors or because one individual weights one part

of the field more than another with resultant differences in level. The contribution of the individual organism is thus an essential part of the theory. Many types of behavior attributed to innate differences in personality may often be traced to differences in level. Thus it has been asserted that some individuals typically overestimate and others underestimate sizes with the implication that such tendencies betray fundamental differences in personality traits. It is possible to transform underestimators into overestimators and vice versa by proper choice of series and comparison stimuli, just as negative time-order errors may be changed into positive time-order errors through manipulation of the stimuli. Further investigation of personality traits may show them to be matters of level capable of change with change in stimulating conditions rather than matters of genetic constitution

Finally, the theory of adaptation-level provides a framework for automatically-operating factors as well as organizational forces in behavior. Since patterning depends upon the position of the neutral region, whatever affects the adaptation-level must be determinative for the organization of behavior Thus so-called irrational factors may have rational effects. Let us consider the following concrete case. If a series of stimuli ranging from 200 to 400 grams is hefted, the stimulus judged "medium" is found to be about 250 grams. If a 900-gram stimulus is added to the series, the medium stimulus moves up to about 313 grams If the 900-gram stimulus is now made a background stimulus by presenting it before each series stimulus is judged, the medium stimulus becomes about 350 grams (Helson, 1947). Mere increase in the number of presentations of the 900 gram stimulus suffices to change the adaptation-level with resultant changes in judgments of all the series stimuli. It would be stretching the concept of "insight" or "understanding" to assert that increase in the number of heftings of the 900-gram stimulus has changed the subjects' understanding of the problem of judging and thereby affected their responses. Indeed, this would be a highly artificial explanation since the judgments are made on the basis of the way the stimuli feel. To take another example, consider the fact that dogs confined for ten minutes in a box suffer far greater traumatic effects than if they are confined for ten one-

minute periods (Frederickson, 1950). Here, as in so many cases, change in a "mechanical" factor, minimized by field theorists, may profoundly affect the behavioral field Such factors, we thus see, may operate at higher behavioral levels to produce important consequences as well as rational factors which patterning theories have stressed.

Our aim in this chapter has been to delineate the main approaches to perception and, so far as possible, to reconcile their positive contributions. By envisaging perception as part of the process of adaptation in the theory of adaptation-level, perception, no less than motivation and learning, is seen to play its part in the economy of the organism in its attempts to adjust to external and internal forces.

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CHAPTER 9

LEARNING

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Introduction.—There is no lack of theories of learning. More pages have been written, more "points of view" formulated, more American psychologists have become famous in the field of learning than in any other comparable topic in psychology.

And yet there is no single theory of learning. Perhaps there cannot be, as we shall see in a moment. There is only a welter of facts, laws, principles, postulates, and generalizations. It is as if the field of learning were a giant mirror. Ask a psychologist what is his view of learning and you will discover his scientific credo, his beliefs and prejudices, his love of empirical fact, of deductive elegance, of operational rigor, of didactic power.

It is not possible to bring any simple order into as large and complicated a field as this in the course of a single chapter. Only by the use of relatively strong-arm methods can any simple arrangement emerge. And in such simplifications, the reader will be told what this particular author thinks is important.

Learning Interests Many Disciplines. Our task is peculiarly complicated by the interest of many different disciplines in the field of learning. The philosopher, for instance, distinguishes between two very broad theories of knowledge, the empirical and the nativistic. The one view holds that all human knowledge is gained through experience, i.e., by learning; the other view maintains that certain ideas are part of the inborn nature of each individual, and learning assumes a secondary role. If learning is the critical issue, then surely

the philosopher should have some say about what constitutes an acceptable theory.

This kind of difficulty does not stop here. The educator, professional or practical, is concerned with what people learn. Schools in the broadest sense must be very old. Each teacher has some kind of a philosophy, some convictions about the processes under his supervision and control. A good many teachers have strong opinions on the subject. Some of these opinions, unfortunately, are demonstrably wrong. In any case, theories about the learning process are inextricably interwoven with the philosophy of education itself.

Older and more widespread than recognizable education are the informal processes of growing up, of acquiring the ways and customs of the family and the community. The sciences of culture in this broad sense, social psychology and cultural anthropology, are rather new, but their claims on psychology are no less insistent than those of the educator. The whole of personality is held to be a product of social forces acting on the child. Political and economic life take their stable form because one person learns from another. Given the rules for the learning process, we ought to be able to shape citizens in the form we want them, to change human institutions into a more ideal model.

Theory Has Been Over-Exploited.— The psychology of learning, and particularly its theoretical side, has been affected in two ways by the strong interest of these varied disciplines. First of all, the philosopher, the educator, the sociologist has each been an entrepreneur promoting his own views of what the learning process ought to look like. Modern scientific efforts to discover the nature of learning have been bent to views which have been chosen principally for their usefulness in these foreign fields. The second result is the complement of the first. Theories propounded by psychologists have found a seller's market. To a greater extent than in other areas of psychology, speculations about learning have been taken as truth and immediately been given wide currency. There is not an important text on educational psychology but what is filled with accepted "principles" of learning.

Such an atmosphere is hardly the most happy one for careful, critical thinking. Theory has produced too many experiments designed to illustrate and demonstrate, rather than to test critically, the consequences of a given view Results have been interpreted in line with the partisan biases which led to the formulation of the experiment in the first place. The total picture of learning that has come out is anything but integrated, coherent, consistent.

Not One Theory, But Theories.—After all, we should not expect to find a single theory of learning. There is not a single theory of electricity or of the weather. Learning is an extremely complex process. We are concerned here with the long-section view of almost all psychological processes *Learning has to do with changes in our experience and in our behavior as a result of an earlier response in a similar situation.* Such changes may involve any aspect of the total response which one makes What is learned may involve the selection and organization of the sensory stimuli; it may involve the needs and drives of the individual, it may concern the selection and patterning of response

In a given experimental situation, as in most practical situations, what is learned largely concerns one or the other of these widely different aspects of behavior And corresponding with the differences in material, there are differences in the amount that can be learned, in the rate at which it is learned, in the role of all of the multitude of conditions ancillary to the learning process Until the learning of each type of material is fully understood, it is too early to prove that they all are examples of one basic process What we have had in the past has been the unsupported assertion by each theorist in turn that the generalization which he has made in terms of limited material is the principle which explains all learning Such generalizations have been decidedly premature.

Historical Background

Five Historical Lines.—To untangle the confused threads of our picture, it will help if we look briefly at the past. In making this survey, I should like to emphasize the close ties among com-

mon-sense observation, formal experiment, and theory. Everyday experience is slowly reduced to exact, laboratory measurement. Both of these generate theories and broad generalizations; and the theories in turn have a great deal to do with what experiments are performed. Science progresses slowly, by small steps, and at each step it is still the heir to both the truth and much of the error of the past.

Some of the ideas about learning have indeed a hoary history. Once they must have reflected common-sense observations. They were simply names and descriptions of what every observant man might know. At a later time, common-sense grew up and was accepted by philosophers. They fitted these observations into formal systems which were then cloaked with authority reflecting the fame of their protagonists. Modern psychologists translated both common-sense observation and philosophical principle into experimental form. Often they couched the older concepts in physiological terms, thereby expressing the hope that in this direction would lie their verification at some future time.

This gradual evolution of our theories has both a good side and a bad one. The good side comes from the fact that few experimental results depend on the experiment alone. Almost always there is a great deal of observing and thinking that goes on before a formal experiment is done to demonstrate a fact. The experiment simply makes a formal record of what the experimenter hoped was true. But the bad side is that some people, accepting too readily the generalizations of the past, have gone on to do experiments with no real effort to test what they took for granted. As a result, the original principle seems to be confirmed by the success of later work that, in fact, never tested it.

I. Memory. Among these common-sense observations is that of *memory*. No one has trouble in understanding what is meant by this word: today I see something, I react to it, it is here now. Tomorrow I can recall it, bring it back so that I can describe it or react to it as though it were here. Experiencing it again, I know that it was there then. Memory, so conceived, is a fact beyond cavil. It is a psychological fact as elementary as response itself.

How shall we explain it? Quite evidently something has been changed by the first experience which persists in some manner until recall takes place. In the simplest view, it was the original sensation itself which somehow persisted in very weak form. The subtle motion set up in the senses did not die out at once but lasted, unnoticed, in the antechamber to consciousness. Such a view was held by the Greek philosophers and was echoed again by men like Hobbes. Another possibility was that some essence of the object, the "chairness" of a chair, for instance, was realized in the mind. Once there, it could be appreciated again by attending to it. Still other philosophers thought of the mind as a waxen tablet, the *tabula rasa*, on which each experience left its mark. Such marks or traces facilitated the flow of new energy through the same path, and revived the former experience.

Today we know many more facts about memory, but our theories have changed much less in the last 150 years. We should reject, quite naturally, such terms as mind, or notions like vibrations of the nerves. But if we look at the form of what the philosophers were saying, we should probably want to say the same things, only in different words. We should say, first of all, that there must be something that *persists*. What it is we simply don't know. It is probably something physiological, or at least it has some kind of a physiological counterpart. And it is probably not any one of the things that people have thought of. For instance, it is not synaptic resistance, the latest bit of pseudo-physiology to cloud the real issue. We know some positive things about this persisting something. It resides in the nervous system It is affected by drugs and probably by a sharp blow on the head. I shall return to this persisting "X" in the discussion below.

A second general point on which modern theory would agree would be that this "X" alone is not sufficient for recall. Other conditions, duplicating in many ways the original situation, must be present if the original experience is to be revived. Often only one element is lacking. We say that memory is *redintegrative;* it fills in the vacant chair, so to speak. Or perhaps there is some need to recall a certain kind of an item. We try to think what it was that

helped us last time in the situation so like the present one. In general, the full responsibility for memory cannot be laid on a special condition within the nervous system; external circumstances which persist or recur are of equal importance.

This is not the place to discuss memory in detail. Just now we are interested in the fact that these rather general properties of memory do stand out in its history. The two we have mentioned will have to suffice.

II. Association. A second common-sense observation is that of *trains* or *sequences of ideas.* In recall, in revery, in dreams, in the free play of thought and imagination one idea follows another in half-ordered, half-disordered sequence. Sometimes our thoughts and images seem to come tumbling into mind, pressing out the earlier ones before we have finished with them. At other times, they come slowly and with effort. However it is, our memories seem to be linked together like kaleidoscopic sausages in a giant string.

Once again the fact has long been evident and the theories to explain it have easily come to hand. Historically, the theories fall into two classes. The older were largely descriptive and had to do with what kinds of items were associated. These are the familiar Aristotelian Laws of Association, the principles of Similarity, Difference, Contiguity in Space, and Contiguity in Time. The connection of one idea with another could be placed in one of these classes, horse and donkey because they are similar, black and white because they are opposites, waves and seashore because they are next to each other in space, and thunder and lightning because one follows the other in time.

But what started out to be largely a description or a classification did not serve so well for an explanation. To explain why just this association occurred in this person at this time required something more. And so there were added the so-called Secondary Laws of Association, the principles of Frequency, Recency, and Vividness. These laws stated that this particular association would be favored over all others because it had occurred the most frequently or the most recently or the most vividly, or, more likely, in some combination of the three.

One point is important here, the relation of the Secondary Laws to the Primary Laws. Just what is it that is frequent or recent or vivid? Evidently it is the previous experience, an experience in which *the two items occurred together* This is contiguity in time. Thus, while any of the Primary Laws might be the basis for an original association, contiguity in time is the one which clearly has the edge over the others in favoring later repetitions of an association. It, and it alone, must always be present if an association is to result from frequent, recent or vivid repetition.

The broad principle of association was the base on which was built a great deal more than a theory of learning As the cornerstone of Empiricism it strongly influenced two and a half centuries of the most active period of English philosophy. But except for the elaboration of the Secondary Laws, the doctrine remained unchanged throughout that period.

Psychology Took Over Associationism. –The most direct translation of the notion of associationism into experimental form is found in Ebbinghaus (1885, 1913) and all of the work which stems from his pioneering studies. Ideas, items, raw and uncontaminated items, were to be found in the nonsense syllables Learning them in long lists created presumably the counterpart of the trains of ideas familiar in everyday observation The Law of Frequency was demonstrated by measuring the gain in learning from reading the list over and over again; the Law of Recency in terms of the curve of forgetting. And the basic character of contiguity as the controlling variable was evidenced by the great superiority of immediate over remote associations. The importance of Ebbinghaus' work lay in adding "how many" and "how much" and "how often" to the older, more loosely expressed relations Ebbinghaus was building experimental science on the framework of James Mill or Thomas Brown.

It is possible to follow the same line of development through the work of many American psychologists. Robinson (1932) frankly talked about his views as being "Association Theory Today" A typical experiment is still that of a man learning a long list of words, or any other sort of items. The task of the learner is to give back the items seriatim. It is always made so difficult that a number of

repetitions are required for its learning. The essential condition of associationism is fulfilled in these experiments, in that items are presented juxtaposed, over and over again.

Linkages established by contiguity need not be limited to ideas alone. Rats running a maze achieve much the same thing. One turn follows the other, first a left, then a right, then around the corner. The various clues, linked into a series, guide a rat (or a man!) in a maze. Presumably the association of these clues with the appropriate response can account for the sequence of behavior.

The Conditioned Reflex Extends Associationism.—It is but a short step from the concept of association to that of the *conditioned reflex,* especially as it was described by the early behaviorists. In terms of theory, this is the right step to take. In the nature of the basic relationship and the conditions under which it is set up, the two concepts are the same; it is only the terms of the relationship, the things operated upon, which have changed. For sensation, we substitute sensory process, or the more ambiguous term stimulus; for idea, we write response. A connection or linkage between stimulus and response is established because they occur frequently at the same time. This is the conditioned reflex of Watson (1919) and his followers.

There are important differences between behaviorism and the earlier associationism. For one thing, the experimental model is rather different. The association experiments had used long series, while a conditioned reflex involved one stimulus, the CS, which is connected with one response, the UR. But Watson lost no time in extending the principle to the maze. Any response sent impulses back to the central nervous system and these in turn could set off the next step in the chain. Response then led to response, as had idea to idea.

Associationism, fitted out in the clothing of the conditioned reflex, is very much alive today. It is best represented by Guthrie (1935), the Spartan simplicity of whose system reminds one of James Mill. One basic principle, he believes, will explain all learning. This is stated thus: "A combination of stimuli which has accompanied a movement will on its recurrence tend to be followed by that movement" (Guthrie, 1935, p. 26). This is associationism,

pure and simple. To understand how it works it is necessary to point out that neither stimulus nor response are just what you or I might think them to be at first. The stimulus, for instance, *may* be the light or sound if it is *simultaneous* with the act. But if the act is delayed, the real stimulus is now some adjustive movement of the head which follows the light or sound. In the same way, a response is rarely just a response. It is not an act, not a running down an alley, not getting out of the box or finding food. A response is only some *movement* that is a part of the total act It is these part movements that are conditioned, and the apparent complexity of learning comes about because a large number of movements have to be successfully conditioned before the total act is learned. Such, in barest outline, is the most modern exposition of this oldest doctrine.

III. Reward Training. A third set of common-sense observations might be tagged, "How to Train Someone." The concept is more sharply defined by the German word *Dressur* which suggests animal training in particular. It is evident that men have understood a good deal about the training of animals for many centuries, certainly for a good part of the time they have had domesticated animals Whenever people have had working animals, like the horse, donkey, bullock, or shepherd dog, as opposed to flocks and herds for food, they evidently had to train them. Curiously enough, the procedures which were used in training animals have excited much less theoretical interest than the related learning in man. Not until we come to modern psychology do we find any account of training methods worthy of serious consideration.

The early experiments which used animals as subjects were done by psychologists such as C. L. Morgan (1894), Thorndike (1911), and Yerkes (1907). Many of these experiments consisted of doing carefully in the laboratory the kind of thing which had been done casually by animal trainers. These psychologists both controlled the conditions and gave more complete descriptions of what went on. But the techniques employed were not new. After all, why did they need to be new? These psychologists wanted animals to learn so they used the techniques that had been successful for so many centuries.

Thorndike's famous puzzle-box experiments with cats are a case in point. A cat was made hungry, it was given a complex task to perform, and when it did what the experimenter wanted it to do, it received food. Under these circumstances it soon learned. It is evident at once that by their very nature the results of these experiments look different from those with human subjects. You can't tell the animal what you want him to do. He has to discover it for himself. You have no access to what the animal was thinking as he solved the problem; in fact you have no access to his "ideas" at all unless you stretch that term. No memories, no words, no evident associations, only the gross fact that a complex act is gradually achieved in less and less time. This is the typical datum of an animal experiment.

The principle which emerges finds likewise little counterpart in association theory. It is what Thorndike (1911) later dubbed the "Law of Effect." It points to the extremely important role played by reward and punishment in the training process. Those activities, or hypothetically those stimulus-response connections, which are rewarded will be stamped in, will tend to recur more frequently. Those activities which are punished will be dropped out. Reward takes the place of simple coincidence in time as the central agent in the learning process.

In a very limited sense the principle is not new. The Law of Effect bears a kind of step-child relationship to the philosophical concept of hedonism. This is the view that people choose to do those things which are pleasant, that they seek pleasure and avoid pain. Now there are a good many difficulties with hedonism as a theory to explain human behavior. Not the least of these is to explain what constitutes pleasure. Hedonism is even less useful in animal behavior where we have no knowledge of the animal's pleasure and pain. What we know about are rewards and punishments, i.e., things he will seek and things he will avoid. Furthermore, pleasure and reward have two quite different functions. The man presumably knows beforehand what will give him pleasure; the goal of pleasure governs his choice of the path to follow. If anything has been learned, it has been accomplished before the hedonistic choice is made. The animal, on the other hand, learns because he

is rewarded; reward is the agent which accomplishes learning. Evidently, the connection between pleasure and reward is quite remote. The Law of Effect is not hedonism restated.

An additional word should be said about the relation of the Law of Effect to the learning of a conditioned response In the early Pavlovian experiments it was possible to conceive of conditioning as an instance of association. In retrospect this seems to have been Pavlov's view and it is evident that it was Watson's. On the other hand, it is equally plausible to look upon the conditioning experiment as an instance of Thorndike's Law of Effect. Food, the reinforcing agent for the conditioned salivary response, is a reward. Shock in the withdrawal experiments is a punishment What is more, well-fed animals could not be conditioned readily. And if you are willing to admit that rewarding a rat for pressing a bar is still conditioning, as Skinner (1938) and Hull (1943) say it is, the parallel becomes even closer.

Terminology, the names that are given to various phenomena, is after all a matter of usage, and so long as all of a group of people use the same terms and understand each other, there can be no objection. If the meanings of words have been changed, it is necessary more than anything else that the uninitiated be given a word of warning. That the concept of conditioning has this chameleon-like character can hardly be denied. Current writers who use the term, notably Hull and Skinner, have changed from the older Pavlovian meaning to a broader and different meaning. The rewarding of an act, not the giving of a signal, is the prototype of a conditioned response. The principle of reinforcement in this neo-conditioning carries most of the burden of explanation, and reinforcement, at heart, is Thorndike's Law of Effect. The experimental formula for conditioning is carried over bodily from the Pavlov model together with a variety of terms like extinction, inhibition, reinforcement. The liaison is a happy one, except when it runs into occasional trouble with its in-laws.

IV. Habit, Skill, and Maturation. For our fourth concept let us look briefly at *habit*. Habit is an old concept and in some hands it has been a very broad one. Anything that a man or animal or even

an inanimate object is accustomed to do has been called a habit. Thus the conformation of an old coat to its owner, the path of a bird in flight, the familiar gait or gestures of a friend have all been termed habitual in an uncritical use of the term. Psychologists, although intrigued by the possibility of a close physical analogy, have wanted to be more sparing. They have usually wanted to distinguish habit from instinct or other inherited dispositions on the one hand, and from intentional, planned activities on the other. Habits are thus acquired dispositions to act in a particular way which lead us to do things in everyday life without forethought and, indeed, often without our being aware of their performance

In their explanation there have been advanced two not entirely consistent principles. The one is the principle of exercise in its most simple form. All matter which has form, certainly all organic matter, has the property of *plasticity*. This means that when an object has been forced or driven into some new form or action, it is so modified that the next time it may be formed in the same manner with less effort Crease a piece of paper and the next fold will follow the line of the old crease. Send an impulse through the brain and the channel it follows is imperceptibly deepened for the impulse to follow.

The alternative is the view that not exercise alone but rather exercise of the *growing* organism results in learning. The analogy to exercise of a muscle is quite close. Activity leads to fatigue; let the muscle rest following activity and it grows large. Neither of the extremes of no exercise or of continuous exercise are optimal. Some plan of alternation of rest and action builds up the muscle to its maximum development. Said Dr. Carpenter 75 years ago, "There is no part of the organism of man in which the *reconstructive activity* is so great, during the whole period of life, as it is in the ganglionic substance of the brain." Dr. Carpenter's physiology is a little unclear, but his message for psychology is unmistakable. Added William James (1890, I, p. 110), "Thus, we notice after exercising our muscles or our brain in a new way, that we can do so no longer at that time; but after a day or two of rest, when we resume the discipline, our increase in skill not seldom surprises us. I have often noticed this in learning a tune; and it has led a German

author to say that we learn to swim during the winter and to skate during the summer."

Of the two notions, it is the latter that has the greater interest for us at this point. The first principle, that of exercise alone, is identical with the associationistic Law of Frequency which we met above. As a matter of fact, the concept of habit almost certainly assisted in the transformation of associationism into behavioral terms.

But the idea of growth induced by stimulation is something quite different. The experimental work in which it can be most plausibly demonstrated lies almost entirely in the field of motor skills. To play golf, to throw a ball, to play the violin, to hammer nails or wield the tools of a hundred trades, these are all learned skills in which neither memory nor associations nor the most attractive reward smooths the path of the novice. And in their laboratory counterparts each such skill is learned most readily with well-spaced practice periods.

This maturational theory continues today to look to physiology and neurology for support. Kappers (1917), a Dutch neurologist, proposed that activity in one nerve tract produced growth toward the active tract in nearby tracts which were still undergoing development. This he called *neurobiotaxis*. Coghill (1929) has pointed out that gross, total movements are the first to occur in embryonic salamanders, and that fine, detailed movements appear later. Is it possible that the same kind of *individuation* may be produced in higher animals as a result of stimulation even when they are adult, and that it is this individuation which makes a skill develop? Wheeler (1929) believes that it is, that the kind of change which takes place here is the basis of improvement in many kinds of learning. For this sort of organic change, he wants to use the term *maturation*.

V. Learning and Understanding. There is a fifth line of development which takes us into a border region where the relations of modern theory and its historical antecedents are less clear. The modern problem concerns the role played by intelligence and understanding in the achievements which are measured as learning. Much

learning is not by rote. The most effective methods of study employ more than repetitive drill; they strive to produce comprehension (Katona, 1940). But what are these intellectual processes, such as understanding and comprehension? Can they, rather than rote drill, account for the more effective processes by which we gain knowledge?

Let me illustrate the case that can be made with a relatively simple example. Most of you who are reading these pages are trying to learn, in the broadest sense, what is written in this book. But it is *only* in a broad sense that you are learning. You are certainly not memorizing these words. (At least I hope you are not!) Your real task is to understand the meaning of what is written here. It is part of your job of making sense out of psychology as a whole. This bit of psychology happens to be written in this particular book, but it need not have been. You could just as well have learned it from other sources in other words. And if you once gain an understanding of the subject, remembering what you have understood will not be much of a chore. If you don't understand it, all the memorizing in the world won't do you much good, for you will forget most of the things you memorize before the ink on your diploma is dry.

This is a rather simple and bald statement of the problem as we would state it today. What about its history? One might expect to find the roots of intelligence and intellect in the philosophy of Rationalism. Yet modern experimental psychology draws almost nothing from the stuffy atmosphere of Kant and Hegel and Schopenhauer.

Actually the problem has a long past. Aristotle talked of *nous*, the noetic soul, which we would probably call intelligence today. It was the active part of the intellect. In the same way Descartes left room for a soul that could *know* and was free to "will" in distinction to the mechanical action of the body and the senses. This one side of the philosophy of Aristotle and Descartes was the parent to Continental philosophy, the tradition of rationalism, of nativism, of the science of the mind as opposed to the science of nature. Intelligence and the "higher" faculties were part of this tradition.

Modern experimental psychology grew out of the opposing line of descent. Helmholtz and Ebbinghaus, Herbart and Wundt clearly preferred the healthy scepticism and analytical bias of English empiricism. This is the tradition which produced associationism, as we

saw above. Following the logic of associationism, there was no room left for intelligence. But Aristotle's *active intellect*, Descartes' *knowing soul*, Leibnitz' *apperception*, perhaps even Locke's *reflection* refer to some psychological reality which will not down. It rises again in unfamiliar clothing to speak for a place in present-day theory.

Curiously, the tremendous modern interest in testing intelligence has produced very little experimental work which serves to clarify the role of intelligence in governing behavior. Psychologists have turned instead to experiments on animals which provide, with simplified examples a useful meaning of the word. The best known of these experiments are those of Köhler (1925) in which, he argued, one could see the operation of *insight* as chimpanzees solved simple problems. The behavior of the animals suggests that they "saw" the relation of a roundabout path to the goal, of a tool in the form of a stick or box to the problem before them. When such a solution occurred for the first time, the chimpanzee can be said to have learned inasmuch as he could readily repeat the solution on a later test But these experiments are interesting not so much as examples of learning as they are illustrations of how intelligence can have an experimental meaning.

There are other ways in which the same point has been made Maier's (1929, 1932) experiments to show what he calls *reasoning* in rats are an example. The rat combines, on a critical trial, the results of two previous experiences to solve a new problem. The problem is so simple that its solution does not do the rat great credit, but the form of more complex processes is there Likewise an experiment by Tolman and Honzik (1930) shows this ability of an animal to manipulate and utilize past experience not only in its original setting but in novel settings as well when the animal is faced with a new task.

We return, then, to our original problem. Do these experiments give any clue to the kind of psychological processes which underlie comprehension and understanding? In answer I should probably say that the word clue had been properly chosen. Our knowledge of this field is woefully incomplete, but we do know enough so that a serious problem is posed for a theory of learning.

Historical Lines and Present Problems.—In picking out five lines of development, I have necessarily stretched the immensely complex past on a Procrustean bed. Fortunately, my purpose has not been to do justice to the past but rather to use it as aid in understanding the present. This brief survey should make quite clear how interwoven are facts and theory. In some cases, notably that of associationism, the experiments have grown directly out of well-established theoretical views. It is hardly surprising, then, that the facts supported the theory. In other cases, new facts have emerged in the course of experiments which have started theory off in a new direction. This is perhaps the case with the concept of "reinforcement" in which new facts tend to make difficulties for old theories. Even here, the real change will come only when someone comes along with a good explanation for the new facts.

The most serious consequence of this intermingling of fact and theory is the inevitable distortion of what appears to be the truth and the whole truth about learning. It happens, for instance, that the number of experiments done with nonsense syllables, or with words arranged in similar unrelated series, is very large. Are we to conclude that such learning is typical of all learning and the generalizations from it are very powerful? Or should we conclude that the theory which gave rise to this kind of experimental work was widely held and that our view of the facts is biased thereby? That question is not easy to answer.

Factual Summary

At this point it will be helpful to outline briefly the more salient facts within the field of learning. Such an outline will be sketchy in the extreme but, by blocking out the full scope of the field, it may help in the formulation of conclusions with regard to learning theory. No particular significance attaches to the order in which the topics are listed. They will follow very roughly the outline of historical views given above

Remembering.—Remembering refers to the fact that certain simple, organized experiences can be recalled after a single presentation, and that these recalled experiences can initiate or direct

action in just the same way as did the original experience. Remembering differs sharply from rote, serial learning in a number of ways. First, the laws governing the intensity of the original impression are mostly laws of sensory or perceptual organization. We remember what we have seen. Second, the experience recalled is a reasonably unitary, coherent content. The memory of a face is not first an eye, then a brow, then a nose; some pattern of the whole comes at once and remains. Further details modify the total when they are added. Third, the variable which tells us whether an experience will be recalled or not is the degree to which the original circumstances are duplicated at the time of recall Thus it is the number and importance of the clues available, rather than the number of original repetitions which is important Rather little good experimental work has been done on memory in recent years. (Cf Woodworth, 1938.)

Serial Learning.—This term is used to designate the familiar sort of learning by heart or memorizing. It is learning in which the details of content are generally well known but in which the selection of particular items and their arrangement are highly arbitrary. Examples are learning a poem, a list of numbers or nonsense syllables, and the vocabulary of a strange language The task for the learner is to build up a series of connections so that item A recalls item B, and item B recalls item C. It is customary today to suppose that the reading of an item is both a response to the preceding item and a stimulus for the following one. In this case what is formed is a stimulus-response connection. The experimental work on serial learning is very extensive (McGeoch, 1942) It covers the interrelations of such variables as the length of list, the amount acquired per repetition, patterning of the list, methods of study, and the exact form of the task upon recall. In each of these cases, we can state that such a material or such a method leads to more rapid learning than does some other material or method. But the basic variable is frequency of repetition; in one way or another the other variables are ancillary to the basic one.

Retroactive Inhibition.—The fact that the learning of a new item displaces and interferes with the retention of an older item is

the most familiar case in a whole family of inhibitory effects. Such interference is "retroactive" only in the sense that its victim is something which was established at an earlier time. No one thinks the present influences the past. Retroactive inhibition is believed to be a very general phenomenon, absent only when the subject does nothing, as in sleep. It is most severe when a very similar list of material provides the interference. By far the larger part of the experiments on retroactive inhibition have employed serial, associative learning, and it may even be that this form of experiment provides especially fertile ground for the appearance of much inhibition. It is fairly clear that nonverbal material shows less inhibition.

The effects of inhibition are mutual. If learning list B causes a loss in the retention of list A, list A also make list B harder to learn. This has been called *proactive inhibition.* Furthermore, if the parts of a long list are thought of as small lists, there will be mutual interference among them. Said in other words, there is *intra*serial as well as *inter*serial inhibition (McGeoch, 1942, pp. 175-188).

Forgetting. The ubiquity of forgetting belies the completeness of our understanding of it. All learning probably undergoes some erosion with the passage of time. The practical questions are how much is there and what can be done to overcome it. Forgetting is unequal for different kinds of material and for different items within the same class. It is at least in part functional, i.e., it is a product of our inability to recall an item under given circumstances. Test the person even later under more favorable conditions and traces of his learning are found. The fact of forgetting does not yield readily to experimental control. This is largely because we conceive it as something happening in time without the forgetter doing much about it. Much of the evidence that is used to "explain" forgetting is evidence not about forgetting but about some related process. The explanations, therefore, reflect these outside factors. Freudians, for instance, relate forgetting to repression. Experts in retroactive inhibition believe forgetting is a matter of interference. The average man just forgets and doesn't think there is much he can do about it; memories are "footprints in the sands of time," and the tide invariably comes in.

Conditioned Reflex.—For the sake of simplicity we shall restrict this term to the classical case of Pavlov (1927). His experiment starts, in principle at least, with some segment of behavior which is elicited by a known stimulus. The sight of food causes salivation, or the shock to the paw causes flexion of the leg. A new and neutral stimulus is introduced for a number of trials just before the usual stimulus. A test then shows that the new, conditioned stimulus has become a signal able to call out the formerly unconditioned reflex without the aid of the normal, unconditioned stimulus. Repetitions of the unconditioned stimulus "reinforce" the conditioned connection; tests made without reinforcement "extinguish" the new bond. Completely extinguished conditioned reflexes recover spontaneously with the passage of time (cf. Hilgard and Marquis, 1940). Such in briefest outline is classical conditioning.

Reward Training.—The classic example of reward training is customarily Thorndike's work with cats and other animals. For several reasons, however, later experiments with the Skinner (1938) lever-box or any device for escape and avoidance training will serve our purposes better. Some response is chosen which the animal performs often enough to be counted, yet far below the maximum possible. Whenever this response occurs, it is followed by some kind of "reinforcement," i.e., by some reward like food, or by the stopping of a noxious stimulus like mild shock. The result is an increase in the rate of responding. Very often the response and the reward have something to do with each other, a rat pokes his head through a hole and finds food, or you withdraw your hand and the shock stops. But there need be no such connection. The sufficient principle is succession in time; the contingency "if A, then B" means nothing more than "when A, then B."

Reward training has been made the basis of three distinct theories, those of Thorndike, Skinner, and Hull. Each of these men makes use of a different set of experimental facts. As a result, any simple exposition is rather difficult. Thorndike's (1932) recent work has been largely with verbal responses and simple rewards like "right" and "wrong." He gives the subject a stimulus word and asks for a response; the theory comes out in terms of S-R bonds. Skinner

(1938) has worked largely with animals and has depended on "emitted" rather than "evoked" behavior. Hull (1943) depended largely upon serial verbal learning and maze learning for his illustrative facts.

The factual material gained in a study of reward training may be grouped roughly under the following headings. First, there are the conditions of reinforcement, the relation of the eventual response to the frequency and distribution in time of the reward. Second, there is the relation of the reward to the animal's motivation. Third, there are a variety of facts, and many more problems, which cluster under the heading of secondary reinforcement. This means that the sound of food, the sight of food, the location of the food are all about as effective in establishing a response as is the ingestion of food itself. Fourth, there are facts associated with the extinction of responses once established. Fifth, there are facts about the nature of the stimuli which may gain effective control over the rewarded behavior, and about their discrimination. These stimuli play the same part as does the conditioned stimulus of classical conditioning

Transfer of Training.—Few practical situations in which we learn are ever repeated exactly. What is learned under one circumstance is almost always applied under another. This is the problem of transfer of training, and it is a very general one. It has been the subject of a good deal of controversy, and it has been illuminated by a rather small number of facts. Early experimental work depreciated its possibilities. James denied, for instance, that memorizing improved our ability to memorize, which is hardly the issue. Thorndike and Woodworth (1901) found that judging squares didn't help much in judging circles, also not too relevant a finding.

The more hopeful view of transfer has often been expressed in other terms than transfer. Some writers talk about the "equivalence of stimuli," whereas Pavlovian conditioning gave us the term "generalization." They mean about the same thing, namely, that if a bell means danger, perhaps a horn or a shout does too. There is a parallel problem which ought to be called "equivalence of *response*," but this end of the S-R link has excited less interest. Perhaps this is because the squiggles that a lever writes on a kymograph look alike

for all responses. In still other cases, there may be "equivalence" of both stimulus and response with the result that there will still be a facilitative effect of one segment of behavior on another without identity of either stimulus or response.

If one thing we do influences another, the result need not be beneficial. Transfer can as well be negative as positive. Negative transfer is in some cases the same thing as retroactive inhibition. In general, any adequate theory of transfer must be able to say what is its relation to various kinds of inhibition.

Maze Learning.—Some of Thorndike's (1911) chickens escaped from maze-like enclosures, and a wide variety of other mazes have been used in the years since Small first put rats in the Hampton Court maze. The learning of mazes has always involved some aspects of both serial learning and reward training. It has been contended, then, that the principles derived from mazes should simply be fitted into these other categories. There is some limited evidence, and it is that which should be mentioned at this point, that there are rather special factors involved in maze learning. These experiments show that part of the process of running a maze depends on the general orientation of the animal in space. In some cases the animal takes a short-cut to the end of the maze, a short-cut that is chosen presumably because the general location of the goal box is known. Similarly, goal-pointing blind alleys cause many errors, blinds directed away from the goal are more readily eliminated (Tolman, 1948). Finally, there are recent experiments that suggest that the learning of a location can be set over against the learning of a response, the second of these being the kind of thing predicted by reward training.

The Acquisition of Skills.— Reference was made in our earlier discussion to the difficulty in analyzing how we gain a skill. This is probably not one but a whole group of problems. On the one hand, the learner's task may be to gain a set of eye-hand or other S-R connections. Sorting mail into boxes, the clerk reads a name and makes a particular movement. Operating a complex machine, we come to associate a given signal with the appropriate movement. But the operation becomes more automatic, the control by a par-

ticular stimulus less evident, as the tempo of the movements increases and the range of response decreases. In playing the piano, typewriting, or sending Morse code the rate of response speeds up until there are a substantial number of overlapping S-R units. All the evidence shows that such skills are achieved only when there is some grouping or fusion of successive sensory cues and of the requisite motor units. We pass finally to the kind of skills best exemplified in a sport in which a single, smooth, nicely timed and graded act is performed in response to some quite complex stimulus situation. This is the skill required in batting a home run, fencing or boxing, or fancy diving and dancing. For help in gaining these last skills a rabbit's foot is worth about as much as the psychologist's sage advice.

Problem Solving. Here again the outline of the main facts have been drawn out in the earlier discussion. It is not clear whether lower animals in puzzle-box situations can learn in any other manner than some kind of simple reward learning. Guthrie (1946), for instance, has emphasized how stereotyped are the responses of cats escaping from his boxes. Adams (1929), on the other hand, reports quite the reverse, and Helson (1927) has reported behavior of a rat to which he was willing to apply the term insight. When the inquiry turns from these lower animals to chimpanzees and humans, there can be little doubt that solutions of a problem are discovered if, indeed, the problem is one in which the solution can be discovered without excessive trial and error. While there has been a good deal of controversy about the interpretation of experiments on problem solving, there cannot be too much controversy about the facts. The learning of a maze has to be by trial-and-error; the animal has no other possibility. Learning the solution of some kinds of problems can take place with the discovery of the essential relations among the parts of the problem.

Learning by Understanding.—At the risk of some slight redundancy, this heading is included in this list even though it involves about the same processes as does problem solving. The difference lies in the way in which the material is presented, and to some extent in the reactions of the subjects. Learning by understanding we shall

take to mean instances in which the complex relations among a set of facts are so presented that the learner need not be in doubt about them. Problems which present a real puzzle and in which the solution is suddenly achieved are useful when working with animals; they seem to reveal more obviously the psychological processes at work in the solution. But they do not represent efficient learning Ideally, education should promote understanding, the easy progression from one evident relation to another. The case for this has been put most cogently by Wertheimer (1945) in his book on *Productive Thinking*. The better illustration of its schoolroom application is to be found in a recent study by Luchins (1942). He found that children drilled in arithmetic, but lacking in understanding, might give correct responses when tested on the drilled material But when compared on new material to children who had been led to understand, their records were much inferior

Learning in Perception and Motivation.—It would be improper to conclude this survey without mentioning at least two areas in which the role of learning is almost certainly large but in which the facts are but poorly understood One of these is perception. Ever since the time of philosophers like Bishop Berkeley, it has been supposed that some aspects of our perceptual world were dependent on learning. The argument has been over the question of how much. Does, for example, our whole experience of depth in visual space depend on learning? Or is the appreciation of depth a native ability which may be modified or elaborated slightly by experience? Since we cannot experimentally limit the use a man makes of his eyes from birth on, we cannot get an answer to this question very easily There certainly seem to be differences among people with regard to what they see To the trained forester the woods are a rich and highly varied pattern, while to the naïve city-dweller they present only expanses of green Just what functions are changed here is still a puzzling matter.

Just as difficult are the changes in another psychological area motivation The gap between the kind of motives to which soap operas and Dorothy Dix columns appeal and the kind of motives discoverable in the animal laboratory is very large. Quite evidently

the former motives are acquired, for they are peculiar to each particular culture. But just how it is that American housewives acquire their special variety of a need for security, or college graduates their peculiar codes of sexual mores are matters that are very far from clear. It may indeed be a matter of "secondary reinforcement" or of Freudian "displacement" but such terms help more in textbook discussions than they do in designating substantial fact. It is quite clear that an adequate theory of learning will some day be accountable for these more obscure borderlands.

Contemporary Theories

The time has come to turn our attention to particular learning theories. It was pointed out above that there is little to limit the possible number of such theories but the ingenuity of the people making them up. For the sake of simplicity in our discussion, we shall classify these theories into four species. Of some there will be one or more sub-species.

I. Reinforcement Theories. According to these theories, the probability that a response will occur is increased when that response has been followed by reinforcement. Reinforcement is roughly the satisfaction of some motive or drive. This statement has purposely been made broad because there are some differences among reinforcement theories, and this general statement is intended to cover them all. Let us look at these sub-species.

Ia. Thorndike's Law of Effect. The oldest is Thorndike's Law of Effect. This is the doctrine that the consequences or after-effects of a stimulus-response connection either strengthen or weaken it. "Many of the consequences which strengthen connections are in the class of satisfiers, a satisfier being defined as a state of affairs which the individual does nothing to avoid, often doing such things as attain and preserve it. . . The consequences which seem to weaken connections or strengthen some different connection are all in the class of annoyers, an annoyer being defined as a state of affairs which the animal avoids or changes." (Thorndike, 1931, p. 36.) Stated simply then, a stimulus-response connection will be strengthened

when it is followed by a satisfier; it will be weakened when followed by an annoyer.

Ib. Hull's Law of Reinforcement. The position of Hull follows in substance that of Thorndike. Hull has tried to include a good deal more than the Law of Effect in his basic postulate. Says Hull, "Whenever a reaction takes place in temporal contiguity with an afferent receptor impulse resulting from the impact upon a receptor of stimulus energy, and this conjunction is followed closely by the diminution of a need (and the associated diminution in the drive and in the drive receptor discharge), there will result an increment in the tendency for that stimulus on subsequent occasions to evoke that reaction. This is the 'law' of primary reinforcement" (Hull, 1943, p. 71).

Ic. Skinner's Reinforcement of a Conditioned Reflex The third variation of reinforcement theory is provided by Skinner. A conditioned reflex, he tells us, is distinguished in that it "did not exist until the operation of reinforcement had been performed" and "through elicitation without reinforcement it is removed from the repertory of the organism" (Skinner, 1938, p. 61) Reinforcement requires "the presentation of a certain kind of stimulus in a temporal relation with either a stimulus or a response. A reinforcing stimulus is defined as such by its power to produce the resulting change" (Skinner, 1938, p 62). Lest anyone confuse this view with the classical notion of conditioning, it should be pointed out that Skinner clearly distinguishes between the functions of a stimulus in (1) eliciting a response, (2) setting the occasion, discriminatively, for a response, (3) changing the likelihood that a response will occur, i.e., reinforcing a response, and (4) acting as an emotional stimulus to facilitate or inhibit the established response But this is about all Skinner has to say about reinforcement. Some stimuli reinforce. That is the Alpha and the Omega of reinforcement, except that a reader observes that food reinforces the hungry animal, water, the thirsty animal, and perhaps freedom from shock, the shocked animal. Without going into the types of conditioning and other aspects of Skinner's system, it becomes clear that this principle belongs in the category with the principles of Hull and of Thorndike.

A principle of reinforcement, once established, is quite powerful as a basic tenet of a system. It is derived directly from the results of a large body of experiments on animal learning, notably those I have termed reward training. In the hands of Hull and Skinner it has been possible to include classical conditioning. After all, food and shock, the classical instances of an unconditioned stimulus, do have other functions than that of eliciting a response. They satisfy and annoy as well. Furthermore, maze learning and problem solving require some kind of reward, food in the goal box, for instance, in order that an animal will learn them. Presumably, then, their learning may be due to the "reinforcement" of the correct responses. Finally, it can be shown that the serial learning of human subjects is influenced by reward-like factors, the instructions to the subject, his intention to learn, special incentives, and the satisfactions he gets on accomplishing the task.

An Experimental Example. Periodic Reinforcement. To understand the way in which reinforcement theory may be applied, it will be useful to examine briefly an experimental concept developed by Skinner, that of periodic reinforcement. In this instance, not every response is followed by reinforcement, as is true in the more conventional conditioning procedure, but the first response in every period of, let us say, five minutes (Skinner, 1938, Chap. 4). What is the consequence of this kind of treatment?

The situation is the typical Skinner box with a rat and a bar to be pressed and a feeding mechanism. With suitable preliminary training, the rat will occasionally press the bar, perhaps once every ten or fifteen minutes, without receiving food. Let one response be rewarded and the rate of responding suddenly jumps. A single reinforcement has increased the strength of the response to a fairly high value. If two or three reinforcements were given, the rate would reach a maximum limited only by how fast the rat could chew up and swallow food. But under the periodic procedure, there is only a single reinforcement and then no more for five minutes. Extinction soon sets in, and the rate falls off, only to be followed at the end of the interval by another single reinforcement, another jump in the rate to a slightly higher value, and a further brief curve

of extinction. Within limits, each of these small curves could have been predicted from the usual experiments

The interesting result of the experiment is that after the fourth or fifth spaced reinforcement, a stable rate of responding is achieved. Reinforcements and extinction seem to accumulate and balance each other to give a steady rate of pressing the bar in which the particular response which is reinforced can no longer be distinguished. The rate is highly stable, being reproduced in one experiment almost precisely on 24 successive days. Furthermore, this steady rate of responding will depend on how often the reinforcement is given. If the rat is fed frequently, he will be willing to bang the bar a lot more often; if food appears less often, his enthusiasm wanes. In the less colorful terms of actual responses, a set of rats reinforced every three minutes averaged 319 responses in an hour; reinforced every five minutes, 266 responses per hour; every seven minutes, 220 responses; and every nine minutes, 169 responses

But here a surprising constancy appears The 3-minute rats were obviously reinforced 20 times in the hour and the 9-minute rats about 7 times. What happens if we inquire how many extinction trials there are for each reinforcement? Dividing the results out for the above four groups, it turns out that the four groups gave 16, 22, 25, and 25 responses respectively for each reinforcement The low ratio of 16 for the 3-minute group is traceable to an unavoidable factor in the experimental procedure Neglecting that figure it appears that the animals were willing to push the bar about 25 times for each pellet of food. This ratio, the "extinction ratio," holds constant whether the pellets come fast or come slow. This is, in a sense, what we said before. Each reinforcement builds up the strength of the response. Each unreinforced response cuts it down. For these animals a balance is reached when the ratio is one reinforcement to 24 extinction trials.

There is not time here to follow out the uses to which the extinction ratio can be put. It serves, for instance, as a measure of drive, more unrewarded responses occurring when the drive is high. The ratio may also be used to measure such drive-like states as anxiety. It may be useful in showing evidence of discrimination. In any

case, these examples make clear how reinforcement theory works in one experimental setting.

Is Reinforcement Retroactive?—There is another side to the picture. Not all psychologists agree that reinforcement is the basic postulate for learning theory. It presents certain logical difficulties which are not easily cleared up, and there are sets of facts for which it has no ready explanation. Let us look at these troubles briefly.

The first difficulty is caused by the apparently retroactive nature of reinforcement. Something which happens after an act is completed seems to have the power of going back and modifying the mechanism by which the response was achieved. This is evidently out of harmony with all physical and biological principles. There are several possible ways out, each possible or plausible, but for none of which there is much supporting evidence. It is possible, for instance, that the original response was tentative and partial, and that when the reward is sighted, the response becomes strong and complete. This is a little the impression one gets of a rat cautiously exploring a maze, breaking into a full run when an open path lies ahead. Just as plausible is the notion of an after-effect of the original S-R connection. If something like the original impulse continues to reverberate in the brain, the reward or reinforcement might come along and intensify the after-effect, thus producing a more durable trace. Not too different from this last view would be the notion that the reward is the occasion for the rehearsal of the rewarded act, a rehearsal that would take place within the organism, implicitly.

More careful measurement of the time relations involved in reinforcing conditioned responses has helped to clarify if not to solve this problem. Given any sort of primary reinforcement, it is now quite evident that the most effective time interval is a very short one. i.e., the reward should not be delayed by more than about one second if it is to have maximum effect. Longer intervals can be used, but even a five- or ten-second delay slows down learning tremendously. The problem is therefore not nearly as acute as some people thought it was when Thorndike's cats were the prototype of reward training. There the interval was certainly much longer.

One other important development has acted to mitigate the difficulty of retroaction. This is an understanding of the very important role of secondary reinforcement. Recent experiments have shown that almost anything associated with the reward soon comes to have a strong rewarding effect itself. As the animal turns into the home stretch, it sees the goal box, smells the food, hears echoes from the dishes or noises from the mechanism, and very soon it receives the food. After only a very few trials, the sight of the goal box or the sound of the feeding mechanism are quite as effective as the food itself. These substitutes for the ultimate reinforcement often occur earlier and earlier in the learned sequence. It is no longer necessary for the reinforcing effect of the reward to stretch back over a long interval of time. Thus the basic issue is not solved. It can only be solved by a better understanding of the way the nervous system acts. But the problem has at least been whittled down to workable dimensions.

How Is Reinforcement Related to Motivation? Of a somewhat different magnitude is the problem for reinforcement theory of trying to say just what it is that reinforces. Broadly speaking, it is a satisfier that stamps in a connection. But what is a satisfier? If it has to do with the motivation of the animal, what kind of a theory of motivation does one need to assume? Some theories of motivation, such as that of McDougall, would be ready with an elaborate answer but, almost without exception, the advocates of a reinforcement theory have held a much more mechanical and less flexible view of motivation. Thorndike himself seems to have wavered. It is possible to find at least three sets of terms he used to describe the agent in the law of effect. The definition I have quoted above is a careful and mature one. It says rather little. It gives a good working rule for finding out what is a satisfier, the animal's willingness to approach an object or situation. Hull, on the other hand, subscribes to a view of motivation that equates it to internal stimulation, a view that has been discredited in recent years. Hull's arbitrary use of the "drive stimulus" makes some of his system-building easy and elegant. It just doesn't have any support from experimental facts. His further assumption of drive-reduction is more plausible. Skinner retreats into a position which is at least defensible. He simply says,

there are stimuli which reinforce, we can classify and list them, and that is that.

Two things should be noted about the way people look at reinforcement today. The first is that satisfiers keep being related to some concept of motivation. To the extent that a clear account of motivation should emerge, the happier will be the solution of the question of what constitutes reinforcement. The second fact is that emphasis has tended to shift to the definable aspect of the process of "satisfaction," i.e., to the stimulus situation which gives rise to such "satisfaction." In current theories, reinforcement lies in some stimulus, or in some easily defined state which is a result of stimulation.

The recognition of how important is secondary reinforcement lends added emphasis to this second development. Even if it is not always clear just what gives food its power as a primary reinforcing agent, it is clear that it is the sight of the goal box or the sound of the magazine, or some equally clear stimulus event which signals to the animal, "This is it." One has the suspicion that if we completely understood secondary reinforcement, we would understand the whole mechanism. Perhaps all practical reinforcement is secondary reinforcement and the distinction between primary and secondary is artificial. Or, perhaps the mechanism by which a new stimulus becomes a goal object is the very mechanism of learning. These questions are not answered. Until they are one important aspect of reinforcement theory remains seriously incomplete.

Does Reinforcement Equal Reward? – The third major difficulty faced by reinforcement theory is the lack of a clear quantitative relationship between reinforcement and many instances of learning. The problem takes two forms. The first one, the one most argued centers around what is called latent learning. In animal experiments it is possible to show that thorough familiarity with a maze, gained under conditions when there is no greater reinforcement of one part of the maze than there is of another, still has a very beneficial effect when the maze becomes a path to food. Just wandering around in the maze with no goal gave the animal a good deal of information. This was put to good use when the need arose. Apparently learning takes place without reinforcement; why do we need to have recourse to the concept under any circumstances?

A good deal of controversy has covered the issue of latent learning in animals. The issue need not be so clouded for one has only to turn to many instances of incidental memory in humans to find ample evidence for the same phenomenon. You will be able to recall without effort where you were just before you started reading this text. You will be able to recall when you came into this room, what you were doing at the time, and innumerable other items of the same nature. Now where was the reward? Are we going to assume that every act of perception is automatically rewarded? If so, the usefulness of our concept in many animal problems breaks down completely. It seems extremely difficult to escape the conclusion that some aspects of learning are accomplished without the benefit of this particular agent.

There are other facts which argue in the same direction. These have to do with cases in which there is a reward but where the amount that is learned and the size of the reward are quite out of proportion. Very roughly it can be said that there is practically no evidence that the rate of learning depends upon how much reward is given. Any reward, no matter how small, is just as effective as the largest amount that can be offered. Learning depends on all kinds of other amounts. It depends on how many times there has been reinforcement. It depends on the time of reinforcement. It depends on the probability of reinforcement, but not on the *amount* of reinforcement. Large rewards may have some very disturbing effects. They may completely disrupt the experiment. But these are effects of a different order. The broad fact remains, and it strongly suggests that the reason a reward is reinforcing may not be related directly to the reason it is rewarding. There is some other variable at work here not sufficiently specified in reinforcement theory at the present time.

II. Contiguity Theories. Contiguity theories are the modern counterpart of associationism. They hold simply that when two reactions of an organism occur simultaneously, they become linked or associated so that one now serves as a stimulus for or in place of the other. Contiguity theories are much like reinforcement theories in their general form except for the fact that according to a contiguity

theory *any* two items may become associated. There is no longer need for the intervention of a special agent, reinforcement, to stamp in the connection.

It will be helpful to digress a moment to clarify here the relation of the *law of frequency* or *of exercise* to *contiguity*. The law of exercise often assumes a form very like contiguity as it is set out above. If the two are identical they should have only one name. But there is a real question just how much is meant by the term exercise. Let us see whether we want to use it as a descriptive or as an explanatory principle.

As a description of a very general condition of learning there can be no cavil with the *facts* of exercise. When a situation is repeated and each time a response occurs, learning usually takes place. This is true. But it is equally true that learning takes place when a normal person, awake and paying attention to the matter at hand, is able to react in a way that the experimenter can see and interpret meaningfully. These are all supporting conditions, necessary perhaps, but not in themselves sufficient to insure learning. It would be misleading if they were called "laws" of learning. There would be the "law of the normal observer" and the "law of the intelligent experimenter." But the same thing is true of exercise. It is not enough to state that there has been *repetition*, it is necessary to state what has been repeated. According to one view, learning may take place when there is *repetition of reinforcement*. This is not what is usually meant by the law of exercise. Or repetition might refer to repeated occasions for exploration and discovery. This is certainly not what is intended. Actually, when the law of exercise is used as an *explanation*, it commonly means the repeated association of two items. For this meaning it is preferable to use the more precise term, *contiguity*, which points to what is believed to be the central and controlling condition in the learning in question.

IIa. Robinson's Modern Associationism. Let us return to the specific instances of a contiguity theory. Robinson, among recent writers, is responsible for the most explicit statement of the old associationistic doctrine. "The fact that two psychological processes occur together in time or in immediate succession increases the proba-

bility that an associative connection between them will develop—that one process will become the associative instigator of the other" (Robinson, 1932, p. 72). Many sorts of psychological processes may become associated. The instigators may be sensory or perceptual processes, ideas, intentions, and emotions. Once associated, these may instigate other perceptions or ideas, sets, movements, and glandular secretions. The detailed application of this general law requires knowledge and the application of a number of subsidiary laws; the laws of frequency, of intensity, duration, context, acquaintance are examples. A law of assimilation helps account for transfer of training. Robinson is a good deal like Thorndike, but with the law of effect left out.

IIb. Pavlovian Conditioned Reflex. I have already commented on the fact that the Pavlovian conditioned reflex is a logical extension of this same doctrine. What Hilgard and Marquis have called the principle of substitution holds that "a conditioned stimulus, present at the time that an original stimulus evokes a response, will tend on subsequent presentations to evoke that response" (Hilgard and Marquis, 1940, p. 76). The principle is so simple, so clear that it needs no elaboration. One sensory process is associated with another sensory process. On repetition the one process takes the place of the other in calling out the response.

Actually Pavlov was not content with the single, simple principle. "The psychologist," he says, "takes conditioning as a principle of learning, and accepting the principle as not subject to further analysis, not requiring ultimate investigation, he endeavors to apply it to everything and to explain all the individual features of learning as one and the same process. . . . The physiologist proceeds in quite the opposite way" (Pavlov, 1932). Pavlov wants to add such additional concepts as inhibition and induction, concepts which he believes are required by the facts of conditioning. Thus he wants to limit, but not at all to deny, the usefulness of the notion of contiguity.

IIc. Guthrie's Contiguity Theory. Today the important advocate of a contiguity theory is Guthrie. His version of conditioning is more extreme than that of Pavlov himself. "Stimuli acting at the time of

a response tend on their recurrence to evoke that response" (Guthrie, 1934, p. 199). Presumably there is some stimulus which evokes the response originally. This would be the unconditioned or reinforcing stimulus. But this is utterly unimportant in Guthrie's system. All that matters is the "law of simultaneous conditioning or association by contiguity in time" of a stimulus and a response. Furthermore, Guthrie suggests that "the outstanding characteristics of learning, which have been expressed in terms of frequency, intensity, irradiation, temporary extinction, conditioned inhibition, forgetting, forward and backward conditioning, and so on, are all derivable from this more general law" (Guthrie, 1934, pp. 199-200).

At this point the student finds a rather curious state of affairs. Associationism has had two offspring, one a theory, the other a large and respectable body of facts. They are equally legitimate descendants of the parent stem, and yet each denies or ignores the existence of the other and their common paternity. The theory, Guthrie's contiguous conditioning, bolsters its case with facts derived from the conditioned reflex and one or two other animal studies. It depends even more on the very many *assumed* facts which fill in the gaps in the explanations. The factual material on associations, on the other hand, fills a rather large book, McGeoch's (1942) *Psychology of Human Learning*. Here are compiled the results of perhaps a thousand experiments, the larger part of them done in the spirit of traditional associationism. And yet McGeoch concludes, "Contiguity . . . is a necessary condition of learning. The question has often been raised whether it is *sufficient*, whether it alone will produce learning . . . Contiguity, together with frequency, effect, and associative spread, have high relative weights in the determination of learning. . . Contiguity and motivation are jointly acting variables, each powerless without the other" (McGeoch, 1942, pp 540-541).

An Experimental Example: Similarity as a Factor in Retroactive Inhibition.—Let us take a look at some typical experiments. Because the facts and the argument are so clear, a phase of retroactive inhibition has been chosen. Back in 1920 Robinson showed very clearly

in a series of three experiments that it was the kind of material interpolated between learning and recall which determined largely whether forgetting of the original learning took place. He had subjects, for instance, learn lists of numbers. Then, before recalling these numbers, they learned other material such as words, consonants, poetry, or they even studied a second list of numbers before recalling the first. In each case it was the learning of a second list of numbers, a highly similar activity, which had the largest adverse effect. About 30 per cent of the material was forgotten because of studying other numbers in the three-minute interval. Learning poetry or doing mental multiplication has some bad effect but much less, only a 5 per cent loss beyond the amount shown by the controls.

Robinson and others took these results to mean that associations formed in the first period of study were interfered with by other associations formed in the second period of study. Thus the connection $4 \rightarrow 5$ in the first list would be upset because the second list would require the formation of connection $4 \rightarrow 7$ or $4 \rightarrow 9$. With very *dissimilar* material in the second list, the probability of such interfering connections would be relatively small.

Robinson's results left much of the dimension of similarity unexplored. McGeoch and McDonald (1931) filled in the gap with an experiment which showed several clear-cut steps in the degree of similarity, and corresponding steps in the degree of retroactive inhibition. The material used was lists of adjectives. They chose their conditions to produce much larger effects than Robinson had achieved. Learning numbers caused a loss of 26 per cent (regarding the control group as zero loss) to the original adjectives. Learning syllables, slightly more like the adjectives, increased the loss to 42 per cent; unrelated adjectives pushed it to 47 per cent, and synonyms of the original words to 68 per cent. Not satisfied with this gradation, they divided their synonyms into three groups, remotely, moderately and closely related to the originals. The losses due to retroactive inhibition stepped up from 67 to 77 per cent, and finally to 86 per cent with the closest degree of similarity! Presumably two closely related synonyms have in common a large core of associations. In learning a list some of these associations form the bridges

to other words in the list. In the final recall, one starts across one of these bridges only to find an unmarked fork in the road.

The general tenor of this explanation can be carried one step further. Suppose we try to specify the two terms of an association, the stimulus and the response, or the instigator and the instigated. Our theory above would lead us to suppose that identity (or a high degree of similarity) of the *stimulus* member of the pair would cause much greater interference. Such has been shown to be the case in transfer of training. That the same holds for retroactive inhibition was shown by the McGeochs (1937). They had people learn adjectives in *pairs;* one was the stimulus, the other the response to be given. When a synonym for the *stimulus* word was interpolated, the loss was most serious, 35 per cent When the synonym was for the *response* word, the loss was less, only about 20 per cent, no greater than the effect of unrelated adjectives These results permit the conclusion that the entire effect is what McGeoch calls *reproductive inhibition*, i.e., a false association blocking out the true one in the process of recall.

Studies such as these illustrate very well the spirit of the contiguity theory. Their authors have been thinking in contiguity terms and have found that kind of thinking congenial and adequate. These experiments don't prove the contiguity theory. In fact, it is doubtful if any experiments ever can. Contiguity theories are a sort of least common denominator of learning situations To prove them, you have to prove there is nothing else but the factor of contiguity in time. And that is a very big job.

Failure to Learn with Contiguity Alone.—Contiguity theories have been subject to a severe and almost continuous attack over the last thirty years. The earlier disproofs were concerned largely with Watson's formulation of the law of exercise They consisted of demonstrations of the fact that animals often learned mazes *in spite of* frequent repetitions of errors rather than because of frequent repetitions of correct responses. A second flaw in the argument appeared when experiments on conditioning got under way and people found how important were the animal's motivation and his adaptation to the experimental situation But by far the most elaborate attack on

this view has been that of Thorndike and his helpers who set out systematically to test whether sheer contiguity, unaided by effect or other psychological factors, could stamp in a response. It will be worthwhile to describe one of Thorndike's experiments (Thorndike, 1932).

Subjects were asked to write down a long list of nonsense words which were read to them. The reader followed certain rules of pronunciation in saying such words as *wol-auf-ee*, *faig-oul-ize*, or *kocc-eex-ay*. Certain sounds such as the long *a* in *late*, or the short *o* in *for* occurred over and over again. Each sound can be written in several different ways (in English spelling!) so that it was possible to find the way it was put down each of the 49 times it occurred in the list. The analysis of all the results is extensive and complex. The simplest cases are those in which the initial frequency of a single spelling was high. The short *o* was written *O* by most subjects. Of the first ten times the sound was used, *O* was written 8.37 times on the average; in the next ten, 8.00 times. Now if there was any effect of stamping in a particular response as a result of contiguity and frequency, this spelling should have been favored and should occur more frequently toward the end of the list or in a repetition of the list. The third and fourth tens actually gave scores of 8.61 and 8.06, hardly an increase over the first ten. When the first part of the list was repeated on another day, the first ten sounds scored 8.44 and the second ten 8.06, no change at all within the limits of measurement from the first reading.

Combining the results from the three sounds, *s*, *o*, and *a*, all of which occurred in one form with a high relative frequency, one finds 2094 cases of the preferred spelling in the first repetition of the list, 2080 in the second repetition. "Even in this favored case," says Thorndike, ". . . the strong does not gain from the weak. The *status quo* is maintained" (Thorndike, 1932, p. 47). It is not contiguity and frequency alone, it is always contiguity *plus* some other factor, *plus effect*, or *plus belongingness*. Many other writers would agree with Thorndike that there must be a plus to contiguity; they disagree very much about what that plus is.

III. Pattern-of-reaction Theories. This is not a very satisfactory name. There is a group of closely related theories which might be called *perceptual pattern* theories or *field* theories, but such terms are either too specific or they lead attention away from the central problem. *Pattern-of-reaction* theories, then, hold that for a connection to be learned the items to be connected must be responded to in some sort of a unified way. Once an active functional connection has been formed it can be recalled and reused in the future.

The primitive illustration of this is to be found in Rubin's study of figure and ground which showed, in short, that you remember what you "see" and fail to recognize patterns "seen" in a different way (Helson, 1925). More familiar examples occur in our everyday speech. If we speak of "an old man's hat" or the "picture of a storm in the back hall" we suddenly discover that these expressions can be understood in more than one way. Was it the hat of the *old man* or the man's *old hat*? These examples point to an active cognitive process in the mind of the listener. What will be remembered will depend on what was understood, on the patterning of response on the part of the learner. Such patterning processes need not, of course, be perceptual and visual, nor need they be verbal and cognitive. They may involve other aspects of response having to do with motivation or action. Nature provides us with a flux of experience sufficiently ambiguous so that selection and recombination are required at every stage in our response.

IIIa. Köhler's Learning by Insight. The best known of these theories is that of *insight*. This term was used by Köhler to describe the way in which an intelligent chimpanzee solved a typical detour problem (Köhler, 1925). Faced by a barrier, apparently bewildered, the ape's behavior suddenly changed and became relatively direct and effective in using the proper means to get to his goal. It was as if the ape had had an 'Ah ha!' experience. Something within the mental makeup of the animal had changed—in this case it changed suddenly. One can only conclude from his behavior that he now "sees" the situation differently. A stick, which a moment before was an indifferent object at one side of the field, has now become the

bridge to the goal. It has come into a new functional relation to the other parts of the field, a new pattern has emerged.

Although the term insight was first used in connection with experiments on animals, Köhler uses the term in a very much broader sense to refer to an experience of determination or causation, of a sensible connection between percepts. On the other side, many writers have accepted those particular criteria which were useful in the ape experiments as the sole criteria for insight. Thus, suddenness in solving a problem has been made synonymous with insight. A more general discussion of insight would take us too far afield here. We shall have to stick to the earlier, more restricted view presented in the last paragraph. At the same time, it should be made clear that the arbitrary selection of some single criterion like suddenness completely misses the point of the argument. The term *insight* points to a unified, psychological process in which the various elements of the problem come into a sensible relationship to one another. This process has just as much reality as does our perception of the third dimension or of movement.

IIIb. Tolman's Sign significate Theory. In his systematic approach to psychology, Tolman represents a point of view which differs considerably from that of Kohler and the gestalt psychologists (Tolman, 1932). In certain critical aspects of their theories of learning, however, they have a good deal in common. Tolman has called his view a "sign-significate" theory, by which he means that the important aspect of the theory is the connection of a sign, some kind of stimulus or signal from the environment, to some process within the animal which signifies the goal and the means-end relations leading to the goal. The evidence for this goal-signifying process is, of course, anticipatory behavior. The animal hears the dinner bell, or finds himself in the runway to food, and connects this fact to the idea of food and how to get it. He tells the world how fine it will be by licking his chops in anticipation. This part of Tolman's theory is no more than good reporting. This is simply what the animal does.

The critical question has to do with the explanation of how sign, means-end relation, and significate are linked up together. If it is only by sheer contiguity of the Robinson-Watson-Guthrie vari-

ety, there is very little new in Tolman's view. But that is not quite what he says. The whole complex (sign, means-end path, and significate) is called a sign-gestalt. The theory about them is classified by Tolman as a field theory. In the most general case, "learning trials function to determine and define a total complex . . . consisting of immediate discriminanda and manipulanda and more remote discriminanda and manipulanda, plus the direction-distance relations between these two sets of discriminanda and manipulanda" (Tolman, 1932, p. 135).

Now even some of these words are treacherous. Field theory, it is said, can be equated to any reaction to a complex of stimuli; every sensible psychologist knows animals react to many stimuli, *ergo*, field theory means everything and nothing (Moss, 1942, pp. 284-285). Actually, field theory means, to those who can read, the assumption of a continuous medium within which there are objects whose interaction depends on the properties of the respective objects and the state of the medium between them. Few psychologists have seriously tried to apply field theory to psychological problems, and even then in only a general and preliminary sense. If Tolman's theory is a field theory, then he means that sign and significate are separately represented in some unified process, presumably in the nervous system, with an organic, functional connection between the two. Such a view belongs among our pattern of reaction theories.

Brief note should be made of the kinds of evidence which support Tolman's view of the sign-gestalt or means-end expectation as a useful intervening variable. In general this evidence is all of the variety which shows that behavior is not closely stimulus-bound. Krechevsky's "hypotheses" in rats, for instance, represent unified dispositions in the rat to react to certain features of the environment to the exclusion of other features (Krechevsky, 1932). Further, the notion of place learning implies the construction of cognitive maps, a notion independently supported by the evidence for latent learning. Finally, specific reward expectancies are most easily understood if it is assumed that there is a considerable degree of interaction among the sensory processes which occur in sequence as an animal runs down a pathway to food (Tolman, 1948). Such evi-

dence does not prove the theory, but it does make it useful and plausible.

The Role of Motivation in Patterns of Response.—There is another aspect of a pattern-of-response theory which should be considered. This is the role of motivation or a need in organizing and directing behavior. Both the idea of insight and of sign-gestalt tend to make use of a goal as a feature of the psychological situations which they are describing. But in neither theory does the goal have any special or peculiar organizing power not possessed in some degree by other items. Adams (1931), on the other hand, in a sharply formulated analysis of the learning process, gives the current need of the organism a central role in the reorganization of the psychological field. Behavior takes place only if the organism has a need. The animal will do the best he can to satisfy that need, but he often finds an obstacle in his path. If he is to attain his goal, he must see the situation in a different way, rearranged so that there is a possible way to satisfy his need. Such rearrangement is the core of learning. Thus Adams suggests need as a central reference point to which other things in the field are referred, a view to which Tolman would subscribe much of the time. Only latent learning with its indeterminate motivation would pose much of a problem for this view.

Patterns of Behavior in Conditioned Reflex Experiments.—It is again clear how a theory depends in no small measure on the particular kind of evidence which its author thinks most important. Insight as a simple descriptive concept is most applicable to the kind of learning which takes place in problem-solving situations. It is very difficult to discover insight in a Skinner box! In the same way, Tolman's description of sign-gestalt learning fits maze learning very well; it can be applied to other types of animal learning without difficulty; but it is not readily applied to the ordinary rote learning of the human subject. To help understand this relation between fact and theory it is instructive to note the study of conditioning by Zener (1937) out of which grew a theory of conditioning as anticipatory behavior which belongs with the other pattern-of-response theories. Zener used substantially the Pavlovian procedures. But in

addition to counting drops of saliva, he gives a full account of what the dog did on the experimental stand. He even freed the dog from the restraining harness so that it would be easier to tell what the dog was trying to do. Bell evoked saliva in the conditioned animal, but it also caused him to move forward on the platform, look first at the bell, then look down into the food pan. The behavior was that of an animal waiting for food to come, not that of an animal with food in its mouth. The theory of conditioning as a substituted stimulus seems inadequate for the Zener facts. Quite evidently it is not only a question of which theory is right; the whole experimental design has been varied to meet the terms of the theory which the experimenter sought to test.

An Experimental Example: Continuity in the Learning Process.—Among the issues which distinguish pattern-of-response theories from other theories, one of the more instructive is the contrast between "continuity" and "discontinuity" hypotheses. In general, either a reinforcement theory or a contiguity theory assumes that every learning trial adds its little bit to the final result. Since the conditions of learning are mechanical and external to the material learned, each trial is in a sense a forced trial over which the reacting organism has only indirect control. For a pattern-of-response theory, on the other hand, the response of the organism to the relevant stimuli is of prime importance, and learning will take place only if the animal responds, and responds in the proper way. If he does not respond, there will be no learning. Thus there is the possibility of *discontinuity* in the learning process to the extent that there is *discontinuity* in what he does with the situation before him.

The simplest observations on which this latter view depends are the *position habits* with which every discrimination problem is plagued. Part of the trouble is, of course, purely one of procedure. You can't compare two things unless they are separated either in space or in time. Usually it is in space. But the problem is more serious than this. Animals such as rats show almost a passion for responding on the basis of space cues. Let a rat find food on one side of the discrimination box three or four times in succession and he will try this side first for ten, twenty, sometimes forty or fifty

trials, even when they are repeatedly unsuccessful. Good experimenters know this and take precautions to avoid such position habits. But they often don't bother to mention it. Simple alternations, right, left, right, left, are almost as easily formed and also must be discouraged from the start.

These simple facts become the basis of what Krechevsky (1932) called "hypotheses" in rats. He had the rats make a long series of choices in which no one clue was invariably correct. Nevertheless, he found the rats did not respond at random but they chose on the basis of first one clue for thirty or forty times, then switched over rapidly to another clue for a while, and finally tried perhaps a third or fourth. Thus, one rat might go to the right, then alternate between right and left, then choose the lighter alley, and finally the alley with a hurdle. The disposition to act in a given way, evidenced by the systematic choice of that action, was called a hypothesis. Furthermore, in a given strain of rats one can make certain predictions about which hypotheses will be chosen first by most of the rats, and which ones second or third. Spatial hypotheses usually come early; hypotheses about visual clues often come later.

To make the point more explicit, Krechevsky (1938) trained rats in another situation in which he could be sure that the hypothesis chosen in early trials would not involve the patterns of dots that were eventually learned. Under these circumstances the rats could jump successfully to the wrong card for two days without showing any bad effect when the cards were reversed for the final learning. Evidently the rat was not learning anything about the pattern in the early trials because he was not reacting to it. He was learning in these trials, to be sure, but he was learning to avoid repetitive errors and position habits, i.e., he was learning what not to do. The same conclusion was reached by Lashley (1942, pp. 257-259) in an experiment where the preliminary training was to one aspect of the stimulus, *size*. In the second part of the experiment the animals continued to choose between two patterns on the basis of size even when another characteristic, shape, was there. When tested on *shape* alone they showed no effect of the previous training. Lashley (1942) says: "The mechanism of nervous integration is such that when any

complex of stimuli arouses nervous activity, that activity is immediately organized and certain elements or components become dominant for reaction while others become ineffective. . . . In any trial of a training series, only those components of the stimulating situation which are dominant in the organization are associated. Other stimuli which excite the receptors are not associated because the animal is not set to react to them." Lashley's *organization* is very much like our present pattern-of-response. Unfortunately, his account is not sufficiently complete to decide whether he would accord this notion the central position in learning theory.

Criticisms of Pattern-of-Response Theories.—Today there is far from agreement on the facts themselves, let alone the theory. Pattern-of-response theories have been under severe attack, especially from the advocates of reinforcement theories. On the factual level, the argument has been largely over experiments purporting to show latent learning. If reinforcement can be accomplished by secondary stimuli, the critics of latent learning say, then who knows what possible reinforcement there may be in cases where primary reinforcement is absent? Almost any aspect of the learning situation may become rewarding and help to stamp in the required connection. Yes, but note that the verb in these sentences includes "may." What is secondary reinforcement anyway? Why is it so all-powerful in some cases and so hard to find at all in others? Both theories are able to offer explanations of the facts as they see them, but certainly neither theory can rest its case on present facts alone.

The same thing can be said of the other areas of controversy. Some experiments show *continuity* of the learning process; others show *noncontinuity*. Only more facts can clarify the issue. And what of insight and understanding? Can such learning be made the object of sufficiently careful experimental work that doubters will be silenced? Pattern-of-response theories are in some respects the most powerful theories because they start with the most complex assumptions. They suffer on the other hand because it is so hard to prove that these assumptions are necessary, and scientists prefer simple assumptions.

IV. Maturation Theory. It is customary to set maturation off as something opposed to learning. Maturation is change and development which depends on the native growth potential of the organism. Learning is change induced by external stimulation. Such is the common distinction. Some writers, notably Wheeler (1929), have taken the position that the two are not separate but are actually closely related. According to this view, learning is simply maturation which is set off and directed by stimulation. Even in its earliest stages growth requires that the organism derive food and oxygen from its environment. It soon requires physical supports, things to push and pull, light and sounds to set off typical movements. It follows that growth will eventually be limited or encouraged by the quality of the environment in which it takes place.

It is Wheeler's contention that a good deal of learning is an extension of this process. The clearest examples are in the field of skills. Practice a sensitive coordination without rest, and the skill deteriorates. Rest, and the skill recuperates until it actually exceeds the original level. The psychological function improves much as a soft palm or flabby muscle is built up by exercise *and* rest. Exercise means stimulation, and stimulation results in growth.

Unlike the other theories which we have discussed, this concept of stimulation-induced maturation has received very little notice from experimental psychology. For one thing it is not readily subjected to experimental test. The psychology of learning has been preoccupied with particularistic facts, while the results of maturation are likely to be general and consequently harder to measure. Moreover, it is not easy to disentangle this brand of maturation from the effects of inherited dispositions. The inherited factor cannot be modified in any single organism, and the careful breeding of animals is a hard, expensive job.

Nevertheless, the evidence for the role of maturation in learning is not to be neglected. There are the facts about motor learning, to which we shall return in a minute. There are the experiments on the development of behavior, Carmichael's (1926) results with tadpoles, McGraw's (1935) study of the twins, Johnny and Jimmy, and Bird's (1926) study of chicks. All of these make clear that biological

growth underlies such things as our "learning" to walk. They also suggest that (1) some stimulation and exercise must be added on to the purely biological factors for the final skill to develop, and that (2) there is a range over which more practice will make up for less "growth," or more time for growth will take the place of some practice.

Even intelligence, the last stronghold of the nativist in educational circles, shows some dependence on stimulation and experience. Children raised in an extremely dull environment show less than normal growth in intelligence (Stoddard, 1940). Adults put into an equally dull environment (penitentiary) test less and less bright over a period of four or five years. The changes involved are so profound as to suggest something other than ordinary processes of forgetting. This factor may be related to maturation.

Much the clearest experimental case is that of motor learning, in particular the learning of a skill which eventually requires no conscious intervention. Let us consider briefly Snoddy's (1920) description of mirror drawing. The subject sees the path he is trying to trace in a mirror. It is both upside down and much farther away from him than is normal. The initial movements he makes carry him away from the path rather than along it. The harder he tries, the worse he gets. Hand and fingers become tense as he slowly moves along making one mistake after another. The initial trials are instances of blocking, frustration, and muscular tension that gradually spread over the entire arm. Snoddy calls this an *irradiation pattern*. There will be a short period of improvement, but beyond this more practice may lead to greater tension and eventually deterioration in performance.

The picture is much different if a rest period follows or, better yet, if the learning is spaced out with a rest period following each small group of trials. Now the tension has time to wear off. Even more, the subject returns not only relaxed but able often to do the task *even better* than on the previous day. With regular rest periods the learning proceeds rapidly, each trial giving a maximum return for the practice effort invested. These are, of course, the familiar facts showing the superiority of spaced learning. The interesting

thing is that when practice is pushed to rather extreme limits, 500 or 600 repetitions as opposed to one repetition, progress depends entirely upon the number and length of the rest intervals and not at all upon the amount of practice. Learning becomes proportional to recuperation, and presumably to maturation during the period of recuperation.

The description given above is not correct in detail for all instances of spaced learning. Nor is the indicated explanation the only one that has been given for results like those of Snoddy. The evidence on the subject of spacing in learning comes from such widely different sources, ranging from men learning arithmetic to rats learning mazes, and it is everywhere so limited and thin that final conclusions are dangerous. One can say, with Woodworth, "When all these (other) factors are allowed their due weight, there is still the probability of a 'physiological' factor" (Woodworth, 1938, p. 216).

The same conclusion must cover our more general case for stimulation-induced maturation. There is no reason for supposing that certain aspects of learning, such as memory, need depend on maturation. Wheeler, for instance, makes no attempt to extend the concept that far, making use of a gestalt-like redintegration in this area. There remains a large middle ground in which the possibilities of maturation remain, remain unfortunately largely unexplored. Due weight must be given to the case which can be established and an attempt made to fill in the more obvious gap.

Suggestions for a Theory

Having voiced a number of objections to current theories of learning, I shall now venture a summary which may, in some small measure, make up the deficit I may have created. That these suggestions are tentative goes without saying, but let me repeat here the warning about their provisional nature lest the unwary reader be led to believe that this text contains a certifiably greater portion of truth than the next one.

The more important theoretical considerations in the field of learning can be grouped under six headings. Each might be called

a principle, but actually it is more like a cluster of principles. These six headings are (1) *Carry-over,* (2) *Sensory Selectivity,* (3) *Coalescence,* (4) *Behavioral Parsimony,* (5) *Motivational Spread,* and (6) *Response Patterning.* This may appear an odd and bewildering list. It has been made this way on purpose, because it is important that we not identify any one of these headings too closely with any single concept, but keep our minds open to include ideas from divergent sources. To avoid confusion, however, it may be wise to set up a rough lexicon to translate these concepts into terms more familiar. *Carry-over* will be used to refer to the problem of *traces* and *retention* in the most general form. *Sensory Selectivity* refers to sensory *discrimination* and the converse problem of *stimulus equivalence. Coalescence* might as well be termed *associative recall. Behavioral Parsimony* has no ready translation and will have to stand by itself. *Motivational Spread,* on the other hand, represents an attempt to give a specific interpretation of the more familiar notion of *reinforcement.* Finally, *Response Patterning* has been foreshadowed in the discussion above and refers to meaningful or insightful learning

Let us now plunge in without further preliminaries.

Carry-over.—Any theory of learning has to start with some account of the manner in which the consequences of an act or of a state of affairs at one time in the organism's history make themselves felt at a later time, i e , there is some *carry-over.* It would be most convenient if we could call this very general concept by the name *traces,* but this term has many unfortunate associations. It seems to refer too specifically to the view that something is deposited as a kind of visible trail, that some mark is left, some groove cut that can be followed later. It is probably wiser to discard this term traces and to choose a new name for the intended concept

It is not improbable that all organic processes have some degree of *carry-over.* Certainly Jennings (1906) has shown something of this kind in the simplest one-celled animals such as the *Amoeba* and *Stentor.* Skin and muscle in ma are "modifiable in a limited sense. Nervous tissue, so far as we know, differs only in that it possesses this property in a tremendously magnified degree. Unfor-

tunately, our evidence for nervous modification is not direct. We cannot yet see what happens to neurons or nerve tracts; we can see only the changes in gross behavior which result from such modification.

The critical problem for theory is what should be taken as the simplest psychological counterpart of the neural carry-over. In one view, it might be memory, in which an individual relives the past. Or it might be the facts of habits or skills. Upon analysis each of these familiar examples turns out to be relatively complex. *The simplest evidence for carry-over is probably the fact of recognition in the field of memory and the simple facilitation or inhibition of response tendencies in the field of behavior.* The reason for emphasizing these simple cases is to make clear that the carry-over from some previous experience may have either an ever so slight effect on some subsequent experience, in which case we just have a vague feeling of familiarity, or else a very profound effect, as when a faint aroma brings to mind a flood of full-blown memories. While the instances of explicit and complete recall are perhaps the more spectacular, it seems clear that by far the more frequent instances of memory are those in which there is only a filling in of small gaps left in the immediate situation. Memory is basically *redintegrative* in the sense that the carry-over from the past makes possible the filling out of an experience in the present.

Our basic contentions here are two. First, there must be some kind of a carry-over, the properties of which can be learned only from the consequences of the carry-over in behavior. Talk in terms of the "intimacy of synaptic connections" or "neural facilitation" is decidedly premature. Second, the effects of carry-over are to modify and change subsequent behavior or experience, not to produce or reconstruct it. Each new act must stand on its own feet with an adequate "cause" in the present, no matter how elliptical may be our reference to the "cause" in the past.

Neural carry-over is the basis, then, on which I shall erect my theoretical structure.

Sensory Selectivity.—A second problem which faces the learning theorist is to account for the selectivity of the sensory input to

the learner. In human learning, it is possible to think of this as a matter of attention. In a typical experiment the learner is given instructions; he is set so that he will respond to only a very limited aspect of his complex environment. It becomes almost a truism, as almost any exasperated teacher will testify, that you can't learn anything unless you pay attention! But adding a new concept, attention, does not help a great deal. I can only repeat the facts that *intention to learn, a set toward the material, and the selection of the proper aspect of the learning situation appear as important preliminaries to the process of retention* about which I have just been talking.

Turning to experiments with animals, the problem gets somewhat more difficult. At the start of an experiment, the animal does not understand what you want him to learn He cannot be given verbal instructions The first part of the training is invariably a process of getting the animal to respond to the relevant clues in the situation and to disregard the irrelevant ones In formal conditioning, this process is called discrimination or, more accurately, discriminative learning. In other training situations the process is buried under some rubric like "preliminary training" and receives little attention. The important thing is that this narrowing down of the effective stimulus input is a matter of learning. Superficially it looks like a rather different kind of a process from the intentional selecting of which the human learner is capable Fundamentally they are probably the same

A little thought shows at once that the problem of discrimination has its reverse aspect. To the extent that a person or an animal fails to discriminate or select among the stimuli presented to him, he will make the same response to any of a class of undifferentiated stimuli. The elements of such a class have been called *equivalent stimuli*, equivalent in the sense that they call out the same response But often, very often, they are not equivalent in the sense that, *if* the animal had been trained to distinguish them, or *if* the man had been told to pay attention to this other aspect of them, either *could* have told the difference between them. But in neither case did he. In some measure, the problem of transfer of training is a matter of

equivalence of stimuli. To the extent that what we have previously learned can be applied in a series of new situations which we choose to regard as equivalent to the old one, there will be transfer. This is not the whole problem of transfer, but it is part of it.

There is one other way in which the problem of selection is important. This is in recall. If a person has learned, for instance, to do some act when he receives the proper signal, he quite evidently will fail to perform when the signal is absent. And unless the selective mechanism of the organism is properly set there can be just this failure. The proper clue may be present as a physical stimulus but unless the organism picks out the signal either through intent or by virtue of environmental circumstance, neither recall nor response will appear. Thus this selective mechanism helps to decide both what is originally learned and what is later recalled. And in the case of recall, the selection must correspond with the selection in the original learning.

It is not possible to give a simple explanation of how or why these selective processes take place. Not many psychologists would be satisfied, for instance, with a term like *attention* or *set*. These terms are largely descriptive They need to be explained, in turn, by the circumstances which control attention or create sets. And it depends on our point of view what circumstances we think important. When dealing with the human learner, many of the factors governing selection are native. They describe the way the basic perceptual mechanism is built. Again, we see and hear in part what we *want* to see or hear; i e., motivation modifies the relative effectiveness of certain stimuli. Finally, there must be a kind of learning which helps either a rat or a man pay attention to the right things. We notice familiar objects, we are better able to see well-known details. There is such a thing as the practiced eye.

In conclusion, it may be well to say again what was said above. There appear to be a number of widely different instances of sensory selection that influence learning, discrimination in conditioning, preliminary training and hypotheses in mazes, instructions, intention, and sets in human learning. All of these actually have a good deal in common, and all of them must be explained in turn by both

native and learned factors which are as yet incompletely understood. Sensory selection is both an elementary kind of learning itself and at the same time an aspect of more complex learning in which it is a basic component.

Coalescence.—The third principle is that of *coalescence.* The term coalescence has been chosen to describe the process underlying associative learning for one reason, to emphasize the fact that association is not to be taken as a basic concept in learning theory but rather as a secondary or derived concept. In the two previous points it has been argued that certain aspects of learning can be explained without recourse to association. How shall we proceed, then, if we do not assume association as a basic principle?

The facts in need of explanation are those of associative learning. Present two syllables as a pair on a memory drum and one acquires the power of calling up the other. This is the fact. The law of association makes an explanatory principle out of the fact. It states no single condition of the formation of an association between two items other than their contiguity in time.

The fact is undeniable, but the explanation is strange indeed. Simultaneity may well enough be *one* of the necessary conditions for the formation of an association, it is hard to understand how it can be the only one. Imagine two men walking through two opposite gates of a football field at exactly the same instant. Would they later recognize each other? The simultaneous entry of two sensory impulses into a system as complex as the brain would seem by itself to guarantee little more. Only if the two men met, bumped, frightened each other, or interacted in some other way might we expect them to recall the occasion later. Only if the two sensory events entering the central nervous system produce results which mutually interact would we expect an association to be formed. It is this degree of *interaction at the time of an original impression which will be called coalescence.*

To argue that two sensory impressions coalesce makes good sense. The law of association seems to hold that the residue of an action can accomplish something more than the action itself. It seems to say, two unrelated events become related when they no

longer exist What need is there to assume a curious principle which gives to traces or carry-over an important function which it denies to the process itself? It is the more reasonable view to start with coalescence between the original events Then association in its usual form is no longer necessary Let me elaborate this point a bit more fully

The coalescence of two sensory impressions, or other psychological processes, is usually assured by one of the selective processes discussed above In verbal learning for instance, a special set is created as a result of which the learner forms a pair out of the two nonsense syllables or words In classical conditioning the conditioned stimulus is always a sudden and sharply isolated event which falls only just below the level where it would by itself evoke an explicit response In less favorable cases familiarity and frequent repetition increase the probability that interaction will occur In other words, if two impressions are simultaneous *and* if the selective mechanism is set to bring both impressions into a common reacting field, coalescence will occur

Once there has been coalescence we need appeal only to the notion of carry-over The usual redintegrative mechanism is sufficient to explain the result If the subject is given one part of the original experience, he is able to complete the experience to recall the other half of the pair. Recall in such a case brings forth no new unexplainable connection It serves only to revive something which has been previously achieved The "law of association" is thus transformed into the three understandable principles of carry-over, sensory selectivity, and coalescence

Behavioral Parsimony.—Up to this point I have been talking about learning that has to do with what might be called cognitions This term refers to the whole stimulus end of the stimulus-response chain. Memory and association and simple conditioning might all have been lumped together as cognitive learning Of course, such learning has its outcome in some change in behavior, for that is the only way we can study it Conceptually, however, it is perfectly possible to separate the beginning and the end of the stimulus-response process And having treated the first part, the cognitive part,

attention can now be turned to the second part, learning associated with the control and direction of action.

If we ask what a person will do, we have first of all to give some account of his motivation. This problem is treated elsewhere in this book. Let me point out one or two salient points for learning theory. When I say a person is motivated, what I mean is that he will do any one of a certain *class* of things. When I am hungry, I may rummage in my desk drawer hoping to find a candy bar, or I may go down to the refrigerator in search of a palatable snack, or I may call out impatiently to my wife to inquire when dinner will be ready. On the other hand, if I am cold or if I am frightened, or anxious to finish this chapter, I will in each case do any one of three other sets of things, each set or class of actions defining a motive. This is the first characteristic of motivation which is important for learning. The second is that the motive itself does not usually determine which action within the class will take place The motive simply turns on or off the whole class like a master switch, or perhaps better, like a master throttle. The particular action continues to be under the control of particular stimulus conditions, both implicit in the organism and explicit in the environment I rummage in my desk at the office, whereas I call to my wife at home. Hungry rats in a small, barren, yet familiar cage do nothing. Frustrated, dispirited, ignorant men have starved to death with food available but a few miles away. Motivation alone is not a cause of action, it is only one among several conditions of action.

The patterns of behavior which are displayed by both men and animals are sometimes highly stereotyped. As a result both of inborn mechanisms and of learning which subtly supplements the inborn pattern, there is just one thing to do when a particular need arises. This requires, of course, that the environment is such as to support a fixed and uniform pattern of action. The need must be readily satisfied by the stereotyped response. But at least as often, perhaps more often, a response is called for under varying circumstances. I may be anywhere from attic to basement when the dinner bell rings. What happens, what I do, varies with the circumstances. If upstairs, I come down; if down, I come up. This kind of behaving

in a way related to the situation is labeled *goal-directed*. A choice has to be made from among the class of actions called for by the motive of one act that fits the situation.

It is at this point that *behavioral parsimony* becomes important This principle states that a *person, or an animal, will follow the path, or do the act, which is the simplest, or shortest, or quickest, or least effortful way of satisfying the motive* He will not do anything which is obviously unnecessary Stated very broadly, the principle is reasonably obvious In detail it is subject to very many qualifications and specifications. For instance, the person choosing must have some estimate of the length or difficulty of the path to the goal Part of his choice may be governed by the certainty of one path as contrasted with the relative uncertainty of another Other motives, such as boredom or curiosity, may cut across the primary factors determining the choice All these things are very real complications, but there must be the overall, governing assumption that the organism is stingy, that it does not do things it is not required to do.

A final illustration will help to make clear why this principle is important in learning In most animal experiments it is assumed that what has been learned is reflected in performance But this need not always be so. Suppose that a rat in a discrimination box is allowed to correct his errors in making a difficult choice This means that when he finds the path blocked on a wrong choice he can back out and immediately take the correct alley. Now if he has a slight preference for one side of the box, he will always choose the preferred side first. Half the time it turns out to be correct, the other half of the trials are "errors" but they require only the brief retracing out of the preferred but blocked alley to take the other side Had the rat chosen "correctly" the nonpreferred side, he would have made only an insignificant gain and the rat simply does not bother. Interpose in the incorrect alley a delay, a long run, confinement, punishment, or a drop into the net of the jumping stand and the desired discrimination readily appears. The trouble was not with the animal's ability to *learn* the discrimination but with his willingness to perform it. In general, if the behavior of the rat is to show any change for the better as the rat "learns," then the experimental situation must

be such as to offer some gain or saving when the rat performs the newly learned act in preference to the older one.

Motivational Spread.— Having given a preliminary account of the relation of motivation to action, I am now ready to consider the way in which this relation changes with learning. For want of a better term, let me call the basic process *motivational spread.* The facts which have to be explained are those commonly subsumed under the headings of reinforcement and the law of effect.

The basic fact is again comparatively simple. It is just this. A motive is a common agent which makes any one of a class of acts more likely (or less likely) to happen. The class of actions under the control of a given motive is not limited; it may be enlarged. New or different units of behavior which previously had no connection with a motive become connected with it when they occur at about the same time as an old act which satisfies the motive. The exact mechanism by which this is accomplished is far from clear because the way motivation itself works is not clear But the fact that it happens is clear enough. There is no dispute about that. As a guess I may suppose that *motivational spread is the connecting up of a new act and a motive as a result of a process akin to coalescence, only the coalescence in this case is between a motive and an act rather than between two sensory impressions.*

The conditions which govern the operation of motivational spread are, in general, the conditions of reinforcement This applies particularly to the basic time and frequency relations. But the two terms are far from identical. The term reinforcement really refers to an experimental procedure, to whatever happens when a subject is reinforced. More happens in reward training than motivational spread For instance, the giving of a reward often serves as a signal or stimulus which tells the learner something about the situation, i.e., it serves cognitive as well as motivational ends. Classical conditioning is a case in point. Furthermore, with the continuation of reward training, there is usually some simplification or canalization of behavior. On the one hand new and more effective means of satisfying the motive are added to the subject's repertoire, and on the other he drops out old and less efficient procedures. The adding

of the new is a function of motivational spread; the elimination of the old depends on behavioral parsimony. Both aspects of the process are unfortunately obscured when they are lumped together under the description "reinforcement." Motivational spread is intended as the more restricted and specific concept.

In one important way the present concept widens out the usual meaning of reinforcement. As commonly accepted, the notion of reinforcement has little or no place for motivation. Motivation comes in, at best, as a kind of ancillary or supporting condition. The principle of motivational spread, on the other hand, puts the problem of motivation squarely in the center. This is where it belongs. Neither reinforcement nor the law of effect will operate unless the learner is motivated. And once something has been learned by this method, the subject still must be motivated in order to demonstrate the results of the learning. It is only by reference to the motive that the whole process makes sense. It is this point which the concept of motivational spread tries to make clear.

Response Patterning.—For our final principle we return to a concept set forth earlier in the discussion of older theories. This is the idea that in problem-solving and rational learning there is an active process of selection and analysis, of grouping and ordering. The material on which this active process works includes both cognitive information about the environment and response possibilities for patterns of action. The process may properly be thought of as thinking and intelligence, using those words in a carefully delimited sense. *Response patterning, then, refers to the intelligent selection and analysis, grouping and ordering of cognitive information and response possibilities.*

In problem-solving it is the *goal,* or sensed *problem,* which is the focus to which new ideas are related or from which old ideas are detached and lopped off. Insightful learning in animals displays these characteristics at a very elementary level. The critical step involves the selection of a "means" which stands in the necessary "means-end" relation to the goal. Faced with some kinds of problems men think much as do chimpanzees. The requirements of the problem serve as the focal point which operates in the selection of

those aspects of past experience which may prove helpful. In both cases it is the task or goal which plays the dominating role in the organization achieved.

In other situations, the part played by the goal is not so clear. A given problem may set a man thinking, but he may wander far afield. Now, just to get the information about the problem clearly organized seems to become a goal in itself, apart from the original problem.

Rational learning partakes more of this latter character. The task is simply to learn and understand the material. It is a general task since the effort to learn does not specify any particular way of organizing the material. The requirements for grouping and organization seem to proceed from the material itself. Ideas when brought into relation to one another give rise to relational properties which then serve as handles for recall and as bridges to new relationships. An example of Wertheimer's will perhaps help to make this clear.

Suppose that I write, "The old man is a philosopher, and a drunkard." Few of you who read this will accept the sentence as it stands. Some of you may think, "Ah, yes. 'In vino, veritas.' Drunkards philosophize." Or you may conclude, "Philosophy nearly drove me to drink, but I made good my escape in time." Or even, "Both genius and overindulgence are symptoms of underlying instability. Probably schizoid." But the one thing you could not do, certainly if you had any intention of understanding what I meant, was to let the sentence lie.

It has been suggested that the relationships which are achieved as a result of *response patterning* are of many different kinds. It is not easy even to list them because our knowledge of them is incomplete and imperfect. But in outline the list would look something like this. The simplest relations are of a means-end character. They include what Tolman has recently called *cognitive maps*, an appreciation of the spatial relations of paths, barriers and open spaces in the situation facing the animal. For the chimpanzee, the means-end relations also involve objects, most often visually perceived objects, which can form connections to desired yet inaccessible goals.

Men are also very dependent on visually perceived relationships. A great deal of our thinking is expressed in some form of graphic

symbols, and we are forever making errors because we expect the reality which those symbols represent to have the properties of its graphic counterpart. Furthermore, as recent experiments of Michotte have shown, *cause and effect* is a relationship which is directly sensed in this visual world. (Of course, the "true cause" that a physicist would accept may be something very different from the perceived cause.) Ideas, then, that relate to space will tend to be organized in terms of space. They will be above and below, between, around, smooth, fast, continuous, and so forth.

Other relations among ideas may depend on other sensible properties of objects, such as temporal succession, movement, force, or resistance. Clearly, a great deal of our thinking depends on concrete or quasi-concrete properties. But certainly not all. Logical, abstract relations play some role for nearly every adult and a very great role in great men. Thus, superordinate and subordinate, necessary, particular and general, these are characteristics that may appear independent of any concrete representation. When a man can get his ideas put together using relations of this order, his understanding begins to grow at a rapid rate.

Response patterning, like our other five principles, does not operate alone. Once relations are perceived, it is necessary to remember them and to be able to select from those which might be remembered the ones which are useful upon recall. Similarly, having recalled a number of alternative ideas, a person must select that idea or course of action which is economical. In any practical situation, most if not all of the six principles will be at work. If certain experiments seem to lean heavily on one principle for explanation, it is only because the other principles have been minimized or are neglected in the account which is given. Very likely they are still there in the man who is doing the learning.

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CHAPTER 10

THINKING

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Theories

The term thinking has been used to cover wide varieties of problem solutions ranging from autistic thinking, in which the problems are personal and the solutions are often inadequate or illogical, to creative thinking, in which apparently new materials or verbal products are formed and in which the observable stages of solution follow each other in a more or less orderly manner. In spite of this range of behaviors which the term thinking may encompass, it is the purpose of this chapter to describe their areas of communality and to relate these activities to the broader field of learning.

Since the variables relating to thinking have by no means been adequately investigated, it is not surprising that conflicting theoretical positions have been advanced. At one extreme it has been maintained that thinking is dependent only upon the formation and appropriate elicitation of a vast number of simple associations. The other extreme maintains that thinking is dependent upon some *fundamentally different type of process* from that involved in the association of stimuli and responses. Those individuals who advocate the former position might be described as the *uni-process theorists* and those who advance the latter position, the *dual-process theorists.* Even the extremists, however, frequently offer some indication of compromise—the uni-process advocates striving to find some mechanism that will give selectivity, the dual-process advocates offering to submit their higher type process to ontogenetic analysis.

Perhaps no authority has presented so strong a case for the operation of one process throughout all problem solution as has Guthrie (1935). For him, all learning is acquired through a conditioning

process which is fundamentally the same whether the skill acquired is playing tennis or playing chess. As he so clearly puts it, "Thinking occurs where action is blocked but it is none the less a conditioned response to the acting situation, including the situation within as well as the situation without. Thought is as much ruled by habit as is behavior" (p. 230). He believes that tension rises during the blocking period and equilibrium becomes less stable. Each conflicting system tends to call up "associated items, often verbal" which "in turn serve as cues for action which will facilitate or inhibit one or other of the systems" (p. 224). Thus, thinking is the outcome of conflicting action tendencies resulting from past learning and consists of nothing more than the selection of previous associations to try out in the situation.

Thorndike (1931), too, expresses confidence that principles of association are adequate to account for what he terms ideational learning and problem solving. He holds that the same general laws that "explain how a child learns to talk or dress himself and why he gets up in the morning and goes to bed at night also explain how he learns geometry or philosophy and why he succeeds or fails in the most abstruse problems . . ." (p. 160). The proper weighting of the elements in the situation, the preservation of the right relations of the elements, and correct associations explain successful thinking. He concedes that no associationist has yet described the necessarily complex and subtle processes enabling the selection and combination of connections for even so relatively simple a task as understanding a page of a physics textbook. Yet he maintains that the process must differ only in complexity from automatic association-forming.

Still another modern theorist, Hull (1935), looks to conditioning theory to explain thinking and reasoning. In a paper answering the challenge of a dual-process theorist, he derives from stimulus-response principles behavior that meets Maier's criterion of combining past experiences in a new manner. Previously established anticipatory goal responses operate together in one test situation to guide behavior in the "insightful" manner. Perseverative effects of goal responses in the immediately preceding activity provide the explanatory principle in another test situation. He does not pretend to explain all thinking behavior by these derived mechanisms, but

presents them as an example of how uni-process theory can meet the demands of a kind of thinking situation without the addition of "any peculiarly experiential, psychic or configurational factors." Presumably he would expect other types of intelligent behavior to yield to deduction from stimulus-response principles.

In contrast with this array of Behavioristic theorists is a group convinced that associative learning and thinking cannot be considered as belonging to the same continuum. Originally expounded by Plato, this view has taken more sophisticated form in the writings of such modern authorities as Wertheimer, Kohler, Maier, and Goldstein.

Representative of the extremists among the dual-process theorists is Wertheimer (1943), who distinguishes between "ugly thinking"—problem-solving based upon trial and error and "productive-thinking"—solution involving a restructuring of the problem situation. It is with the latter that he primarily concerns himself. Most problem solving of the productive kind starts when the thinker recognizes or "realizes" the problem, when he sees the inner structure of the situation. Structural strains and stresses become apparent, and vectors are set up which determine the steps to be taken to transpose this incomplete situation into one that is structurally complete or relatively so. In Wertheimer's words, "This is quite in contrast to processes in which some steps, some operations coming from various sources and going in various directions, may lead to the solution in a fortuitous zigzag way" (p. 193).

Not all productive thinking, however, consists of a shift from problem recognition to restructuring. These may be but two steps in a longer unified process, or mere recognition that a problem exists may be a productive achievement in itself. In still another type of thinking, usually artistic creation, problem recognition may play little or no role. The process starts "by envisaging some features" in the structural whole to be created, and the forces set up by this envisaged whole drives the worker to an orderly completion. Apparently the human individual instinctively seeks the inner structure of a problem situation if he is unfettered by habits developed through training in blind association and unintelligent trial-and-error

methods. In application, however, Wertheimer seems to aid the innate processes by presenting new material in a wide variety of situations much as Thorndike, Hull, and Guthrie advocate for optimal learning and problem solving.

Of the dual-process theorists, probably none has so deeply influenced American psychology as Kohler (1925). Like Wertheimer, he distinguishes between "intelligent" and "unintelligent" behavior. As Kohler states it (p. 190):

> We can, in our own experience, distinguish sharply between the kind of behavior which from the very beginning arises out of a consideration of the structure of a situation, and one that does not. Only in the former case do we speak of insight, and only that behavior of animals definitely appears to us intelligent which takes account from the beginning of the lay of the land, and proceeds to deal with it in a single, continuous, and definite course.

So clear to him is the distinction that he projects his position into others in the statement: "There is probably no association psychologist who does not, in his own unprejudiced observations, distinguish, and to a certain extent contrast, unintelligent and intelligent behaviors" (p. 2).

Of the American psychologists, Maier (1945) has probably been the most vigorous advocate of a dual process theory. He regards learning and reasoning as discrete and differentiable on the basis of the product of the problem situation. The behavior is "intelligent" or "reasoning" or "productive" if the organism combines previous noncontiguous learnings in a new adaptive manner. The behavior is merely *reproductive* thinking if it results from a transfer of training. The sharpness of the distinction is apparent in his comment: "By distinguishing between equivalent stimuli which grow out of previous learning and those which are achieved in a problem situation, one can more easily determine *where learning leaves off* and reasoning begins in the solving of problems" (p. 350).

Still another dual process theory is that proposed by Goldstein and Scheerer (1941). Their "concrete" and "abstract" attitudes, while broader in scope than learning and thinking, encompass the

behavior ordinarily included by those terms. The concrete attitude is described, in part, as follows (pp. 2-3):

> "We surrender to experiences of an unreflective character: we are confined to the immediate apprehension of the given thing or situation in its particular uniqueness. This apprehension may be by sense or percept, but is never *mediated by discursive reasoning.* Our thinking and acting are directed by the immediate claims which one particular aspect of the object or the outer-world situation makes."

The abstract attitude, in contrast (pp. 3-4):

> ". . . implies conscious activity in the sense of reasoning, awareness and self-account of one's doing We transcend the immediately given situation, the specific aspect or sense impression: we abstract common from particular properties; we are oriented in our actions by a rather conceptual viewpoint, be it a category, a class, or a general meaning under which the particular object before us falls. We detach ourselves from the given impression, and the individual thing represents to us an accidental example or representation of a category "

Within each category there is a range of behavior, but between the two categories there is no continuity. The demarcation between them is not conceived of as a difference in complexity but, rather, abstract behavior requires a "new emergent quality, generically different from the concrete." The normal person is supposed to shift readily from one to the other according to the demands of the situation. The person with organic brain pathology, the young child, and animals are unable to transcend the concrete attitude.

The authorities cited are merely representative of a larger group that might be mentioned as belonging to the two viewpoints. Watson (1924), Carr (1925), McGeoch (1942), and Spence (1938) have vigorously aligned themselves with the uni-process group. Lewin (1946), Katona (1940), and Tolman (1932) subscribe to a dual-process position. The challenges have usually been made by the dual-process group, who have repeatedly reported behavioral observations or experiments which they interpret to demand a

dual-process explanation. The uni-process theorists have responded with their own interpretations of the observations or experiments and some additional observations and experiments of their own. It must be frankly stated, however, that — with the exception of Thorndike—*the uni-process advocates have failed to attack the problem of thinking by programmatic research.*

The Evidence for Insight and Reasoning

In the chapter on thinking in most general textbooks, observational and experimental data are presented which allegedly demonstrate the existence of mechanisms which transcend those of organized past experiences and their transfer. It is of primary importance, therefore, to examine critically the actual data so often and so convincingly presented in paraphrase.

The data most frequently given are those of Köhler's studies of tool behavior in chimpanzees (1925). This work, indeed, appears to be the cornerstone of the dual-process edifice. The behavioral characteristics that seem to have impressed Köhler most in instrumental problem situations with apes are the frequent suddenness of solution, the change in expression on the faces of the animals at the time of solution, and the continuity and directness of the solution process once the key to the situation is discovered. The following account is typical (pp. 31-32):

> "Tschego first tries to reach the fruit with her hand; of course in vain. She then moves back and lies down. . . . Suddenly Tschego leaps to her feet, seizes the stick, and quite adroitly pulls the bananas till they are within reach. In this maneuvre, she immediately places the stick on the *farther* side of the bananas."

It is on the basis of such evidence that Kohler formulated a theory of insight learning. It is obvious that such data are inadequate to demonstrate the existence of a rational process independent of or transcending past experience. Köhler's data are observational, not experimental, obtained on animals whose past histories were not known. The data on which the theory is based are admittedly selected. In the actual behavorial descriptions in the text, exploration

and manipulation of the situation are more the rule than the exception, as are incomplete and faulty solutions

A wealth of experimental and observational literature amassed subsequent to the publication of Kohler's studies throws into question the adequacy of his interpretations A number of investigators have been able to study Kohler-type stick problem behavior in chimpanzees with known previous experience, and no one has yet reported sudden insightful behavior in apes having no previous opportunity to use sticks in problem situations Thus, Jackson (1942) reported that a young chimpanzee given 10 five-minute trials on a single stick

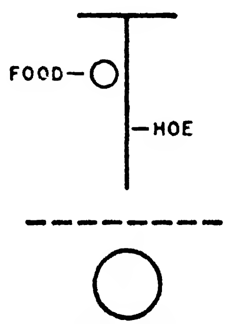

FIG. 10 1. The hoe problem, showing the position of the subject, hoe and food (From H G Birch, *J comp Psychol* 1945 38 367 383, by permission of the author and the American Psychological Association)

problem "failed utterly, making no attempt to use the stick to get the food."

Birch (1945) later presented to six four- to five-year-old chimpanzees, born and reared in the laboratory, the hoe problem with the food lying near the handle and within the blade, as shown in Figure 10 1 Four of the six failed to solve this extremely simple problem within a thirty-minute period One animal succeeded after "his thrashing arm happened accidentally to brush against the stick, causing it to move" (p. 373), and the sixth animal solved the problem in the time period, but it was noted that *prior to the experiments* he had "regularly used sticks on numerous occasions" (p. 371).

When the chimpanzees were subsequently given a three-day opportunity for stick play, they "gradually integrated the sticks into

their previously existent reaching patterns, with the stick coming to be used as a functional extension of the arm" (p. 375). Following the stick play, the animals showed improved performance on the simple hoe problem and ability, after additional practice, to use sticks in a number of new and more complex instrumental problem situations. The degree to which this achievement depended on generalization from preceding experience is suggested by the fact that on the first problem of the new series, a straight-stick problem, the only animal to succeed immediately was the one *who had used sticks adaptively in his cage before the experiments were begun.* Three of the chimpanzees "looked out at the stick and food, and then reached out and pulled in the straight stick that extended from the grill to the food, in exactly the same manner as that in which they had pulled in the strings or the hoe" (p. 377). These animals were obviously responding initially not in terms of any gestalt dynamic processes arising from the structure of the situation, but in terms of their previous experience with instrumental problems.

A more extensive investigation of instrumental problem solving in 25 chimpanzees of one to fifteen years of age was done by Schiller (1949), using a series of 12 stick problems of graduated difficulty. The youngest animals were given experience on a baited single-string problem. The three- to eight-year-old animals began with the simplest stick problem—the rake with food lying between the blade and the animal. The oldest animals started with a single-stick problem. The series progressed through two-stick problems, problems with food and sticks presented on separate platforms in various positions with respect to the animal, and, finally, a stick-joining problem.

All the subjects, including the oldest, learned gradually and by "specific experience." The one- to two-year-olds required several hundred trials to master each of the easiest problems and never learned the problems requiring an adjustment of the stick to the location of food behind or beside it. Their performance appeared to Schiller to resemble conditioning and was easily extinguished by 10 to 20 trials during which the food was attached to the floor or the rake fell apart. The three- to four-year-olds learned in the same manner, showed extinction, but less readily, and required about 100

trials to master each of the problems up to the presentation of food in front of the cage and the stick within the cage. These animals never mastered problems beyond this level. As an example of their failure to adapt in repeated presentations of the two-platform situation, it is reported that they occasionally picked up the stick, bit it, chewed it, but usually dropped it before turning toward the food. They would then reach for the food with the naked hand.

The five- to eight-year-olds learned the simple and even the more complex problems in about 20 trials each, and showed no difficulty with multiple platforms. Apparently not the optical separation but the length of the sequence of necessary responses accounted for the difficulty of these problems. Even the nine- to fifteen-year-olds failed on initial presentation to solve their first problem—the straight stick with food behind it. All reached for the food across the stick and picked up the stick only after hesitation. They pulled the stick without reference to the food, licked it, smelled it, chewed it. Only much later did they put the stick out and push toward the food. On the third or fourth exposure to the problem, they suddenly swept or angled for the food.

In summary, it may be stated that there is no more evidence that naive subhuman primates solve instrumental problems suddenly than there is that they solve discrimination problems or oddity problems insightfully. With experience in many instrumental problem situations the subhuman primates may master new instrumental problems with few or no errors, just as the monkey may learn to solve new oddity-principle problems insightfully, i.e., *with no errors* on initial presentation.

Even though confirmation of an unlearned insight process is lacking in apes, it might be supposed that such an ability is found in human subjects. Adaptations of the Köhler experiments have been conducted on children. Alpert (1928) presented a series of five stacking problems and four stick problems to 19- to 49-month-old nursery school children. Of the 40 subjects given the initial problem of placing a block under a suspended toy and mounting the block, 23 succeeded in the allotted five exposures of 1 to 16 minutes each over a five-day period. The average for the successful children was 2.3 exposures. Of the 23 successes, 1 was immediate, and 5

followed primitive reaching, while the remaining 17 followed varying amounts of exploration and elimination. Only successful subjects continued to the second problem, which substituted a chair for the block. All solved it and all except one did so immediately or after primitive reaching. Later problems changed by substituting a shelf for the suspension, or by requiring turning the block on its side or stacking it atop a box.

The stick series which followed included a simple straight-stick problem, a broom substitution for the stick, a two-stick problem, and a stick-joining problem. Of 41 children presented the first stick problem (most of these subjects had served in the stacking series) 3 solved the problem immediately, 1 after primitive reaching, and 24 after exploration and elimination In the total series of 248 problem presentations, 33 per cent were solved immediately, 28 per cent were failed, and the remaining 39 per cent were solved after exploration and elimination. These results, it should be noted, were obtained on children who undoubtedly had had previous play experience with blocks, chairs, boxes, and sticks, and probably had used at least some of these objects as tools to obtain treasures beyond arm's reach.

Matheson (1931) presented five Kohler-type problems to a group of 28 nursery school children ranging in age from two to four years and found only 5 immediate solutions in 140 problem presentations. In 39 instances, solutions followed manipulation and exploration; in 10 cases there occurred a solution other than the expected one; in 4 cases the solutions came apparently without the expectation of the child Manipulation with or without solution occurred in about two-thirds of the trials. No child under 2½ years solved a single problem, and frequency of solution increased regularly with age.

Both Alpert and Matheson stress that "insight" did not always precede the solution, but often was coincidental with or subsequent to the solution. These investigators are also in agreement that the "insight" or understanding that does occur is of varying degrees in different children and on different problems. Köhler's conception of insight as complete understanding is, according to Matheson, but the upper extreme of the distribution.

The psychologist who would demonstrate in human adults a process of insight or reasoning independent of experience puts himself in a vulnerable position, since the learning histories of his subjects are long, varied, and unknown. Yet studies have been reported on college students that purport to illustrate the operation of a special reasoning process. One study of this type is Maier's "hatrack" problem (1945), which is described by the experimenter as indicating the solution of a problem through *productive* as contrasted with *reproductive* thinking. The experiment was conducted on 75 college students divided into three groups. This problem involved the construction of a hatrack by fastening two poles together with a large C-clamp and wedging them between the floor and the ceiling. Groups I and II were given experience with the materials by helping the experimenter build the apparatus for the Maier string problem (pendulum problem). That apparatus consisted of two units, each having a string suspended from a horizontal pole placed against the ceiling and supported by two poles constructed by fastening two short poles together with a C-clamp as in the hatrack problem. These structures were present while Group I attacked the hatrack problem and absent while Group II worked. Group III was given only the hatrack problem. A fourth group of 180 subjects, referred to as a control group, watched the experimenter construct the hatrack and then was asked to write all the functions the apparatus could possibly serve. Successful solutions were attained by 72 per cent of Group I subjects, 48 per cent of Group II, and 24 per cent of Group III, whereas, even by "the most generous interpretation," only 24 per cent of the "control" group could be considered to have solved the problem.

Maier argues: "If the solution of the string problem contains an element which is equivalent to the solution of the hatrack problem, then a presentation of the element itself ought to be experienced as having a function similar to the hatrack." He goes on to say: "If a greater percentage [of the control than the Group I or II subjects] solve the problem, then it must be conceded that something additional must be added to our explanation of problem solving behavior' (p. 353).

Maier's argument is based on the premise that constructing an apparatus to fulfill some function is directly comparable to assigning a function to an apparatus already constructed. Aside from the fact that one is a manipulation problem and the other a verbal task, there are other basic differences that would seem to preclude the assumption of comparability. The construction task is a circumscribed one: certain materials are provided and a clearly defined goal is set, attainment of which can be directly evaluated by the subject. Trial-and-error methods can be utilized until the subject reaches the *one solution* or withdraws in failure. The verbal task, on the other hand, has multiple solutions and only a partially delimited goal. There is no way for the subject to know when he has solved the task to the experimenter's satisfaction. A subject faced with a specific task of finding a place to hang his hat would certainly have less difficulty than Maier's "control" subjects in finding the desired function for the unconventional hatrack! The results actually confirm to some extent that a difference exists between the problems, for men succeeded twice as frequently as women in solving the construction problem and women twice as frequently as men in solving the verbal problem.

All the data which Maier presents in his experiment support the view that the important factor in determining success or failure is the previous experience. Group I, the group with experience and available cues, made once and a half as many solutions as Group II, which in turn made twice as many solutions as Group III, the subjects without specific previous training. Furthermore, the sex differences in success in all three groups suggest differential experience prior to the experiment as an influential factor in determining solution.

Even though it has not been demonstrated that ape or man has a unique type of thinking, insight, or reasoning process independent of experience, the possibility of this higher type of learning cannot automatically be denied rodents.

Insight or inferential expectation in rats was described by Tolman and Honzik (1930) in a series of experiments, the first two of which failed to yield evidence for insight. The third experiment, IIIA, is the one usually reported as demonstrating insight. The

apparatus used was an elevated maze (see Figure 10.2) consisting essentially of three alternative paths to a goal, a short Path I, a longer Path 2, and a longest Path 3. Path 1 could be blocked at A or E, and the common segment of Paths 1 and 2 could be blocked at B. On the critical trial with Block B in place, 14 of 15 rats chose Path 3. But since 12 previous runs had been given with blocks at E and B instead of the usual A, the authors decided that the results

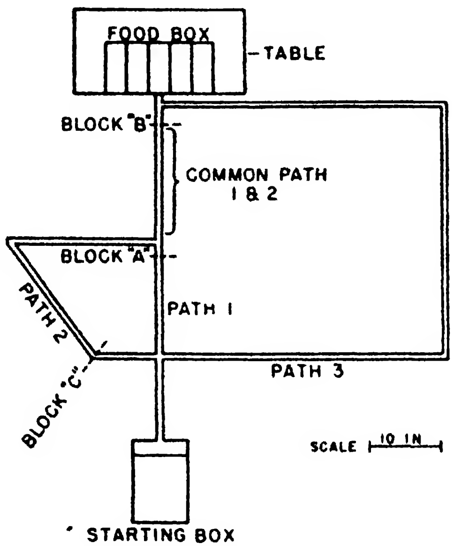

FIG 10.2. Elevated maze used by Tolman and Honzig in Experiment IIIA (Redrawn and simplified from E. C Tolman, *Purposive Behavior in Animals and Men*, 1932, Appleton-Century Co, by permission of the author and the University of California Publications in Psychology)

might be explained in terms of direct training Subsequently they ran Experiment IIIB, differing only from IIIA in the omission of training trials with Blocks E and B. On the critical trial, 7 of the 10 rats chose the longer path, a result failing of significance at the 10 per cent confidence level. Furthermore, the effect of suddenly changing the length of the initial run of all Path 1 trials from Block A in IIIB, or from Block A or E in IIIA, to the more distant point, Block B, was never controlled. It is possible that any change in the preliminary portion of a run might produce disruption of behavior,

some spontaneous searching tendency, or even consistent avoidance of a previously acquired habit.

Using a similar type of maze, Evans (1936) ran a series of experiments including a control experiment in which the regular Path 1 block was near the entrance and a distant block (Block B of the Tolman and Honzik investigation) was later introduced just *before* the point of the common segment of Paths 1 and 2. Under this condition, 46 per cent of the rats immediately chose the longer path even though Path 2 was never blocked, and 67 per cent of these did not relinquish Path 3 in 10 trials.

Such data, of course, neither prove nor disprove the Tolman and Honzik experiment. Since, however, both Tolman and Honzik experiments were inadequately controlled, and the better controlled of the two did not give results which are statistically significant, they cannot be considered satisfactory evidence for inferential expectation or insight in the rat.

The most ardent advocate of rodent reasoning has been Maier, who devised a series of highly ingenious experiments (1937, 1938) which presumably demonstrate that the rat is capable of "reasoning" as well as mere learning. Since all the experiments conform to a general type and utilize apparatus differing essentially only in complexity, they may be illustrated by Maier's simplest situation. This consists of three elevated alleys radiating from a common point and terminating in differently-shaped goal boxes which serve also as the starting boxes. On a particular trial the animal is fed in the goal box correct for that trial, then placed in another goal box from which he runs to either the rewarded or unrewarded goal box. On subsequent trials, run one per day, the rewarded box and the starting box are systematically varied.

Naive nine-month-old rats average about 90 per cent correct choices on their first 20 trials in this situation. Perfect performances are not rare. Such behavior suggests that a simple task is involved.

Maier assumes that the animals solve the problem by joining together two noncontiguous experiences: I, general familiarity with the apparatus, and II, reward of a particular goal box. As far as the present author knows, neither Maier nor his critics ever attempted to analyze the actual cues operating in the problem solution. There is

no evidence that the cues provided by Experience II are the visual ones intended by the experimenter rather than the spatial ones afforded by the position of the various goal boxes in the total experimental situation.

Let us examine the "reasoning" operations in terms of discrimination learning. The animal is familiarized with all stimuli in a spatial discrimination situation, Experience I. In Experience II he is given a single training trial with one of the multiple stimuli to be discriminated. Next, all of the stimuli are presented, and the effect of the previous single training trial is measured. The fact that the rat can profit from reinforcement of a singly presented stimulus in a spatial discrimination problem is interesting but of no unusual theoretical import. Many investigators have shown that animals given training with a single stimulus may demonstrate gain when subsequently tested in a multiple stimulus situation.

Sequences in the Thinking Process

The problem of analyzing the sequences in the thinking process has long been intriguing to man. Those who have ventured to write their analyses have tended, perhaps because of their own superior abilities and their training in problem solving, to idealize the logic and orderliness of human thinking. The results have often been to foster retention of the ancient Platonic dichotomy of the rational mind versus the animal mind, a concept that can be depended upon to appeal to the security-seeking human animal. Yet cumulating research forces the conclusion that even superior adults fall short of the philosophical portrayals of their thinking.

The analysis most frequently quoted and, presumably, most acceptable to psychologists, has been that of Dewey, who outlined the steps in thinking as follows (1933, p. 107).

I. Suggestions in which the mind leaps forward to a possible solution;
II. Intellectualization of the difficulty into a problem;
III. Hypothesis, the use of one idea after another as a leading idea;

IV Reasoning, the mental elaboration of the idea or supposition,
V Verification or experimental corroboration obtained by testing the hypothesis

Dewey recognized that individuals do not necessarily progress through these phases in a set order But even with this qualification the analysis suggests a high level of orderliness inconsistent with experimental data Furthermore it does not describe (nor was such intended) the stages of problem solution for children or subhuman animals

Any classification of the stages of thinking or complex problem solution must be arbitrary, but it is our opinion that the experimental literature on subhuman animals human children and adults suggests a more objective sequence applicable to all The sequence is thought of as being initiated by a problem defined as

I Perception of a situation with incentive or goal inaccessible immediately, leading to
II Elicitation of initial alternative responses explicit or implicit and ranging from unlearned responses to a limited number of highly organized response tendencies these in turn leading either to
IIIa Problem solution (i e goal attainment) failure (withdrawal) or
IIIb Additional reaction tendencies which again lead to either problem solution withdrawal or the arousal of additional reaction tendencies

The goal of a food incentive in a detour situation for example may lead in a hungry chicken to no other reaction tendencies than random although vigorous forward and sidewise movements and when these fail the chicken may withdraw A dog faced with the same situation may first respond with similar random movements but when these fail he may even orient from the food and attempt to encircle the barrier by running around to the left and if this in turn fails he may reverse this activity and essay circling in the opposite direction

A human being presented the Goldstein task of sorting a multitude of objects according to different classifications may quickly

demonstrate two or three highly organized reaction tendencies such as classifying by use, by color or by material (even though he may not be able to verbalize the classifications), and if these responses fail he may produce additional reactions, such as size or form classification, or withdraw from the situation.

Even these simple examples present a degree of orderliness which is frequently not found in problem solutions. The human being may start by manipulating the objects, concerning himself with some activity presumably unrelated to the problem situation, or engaging in spectator behavior before any organized reaction tendency is apparent (there *may* be implicit responses going on, of course), and similar random, nongoal directed behavior may appear or reappear at any stage. Furthermore, the final stage of problem solving, Dewey's verification, may show a distressing degree of disorderliness in which the subject reaches an adequate solution without recognizing his success, or in which he responds as if the problem were solved although the experimenter is unwilling to accept the behavior as true problem solution. The opinions expressed by various psychologists of the theories of other psychologists suggest that inadequate solution or inadequate verification often appears at a high level of intellectual sophistication

There are differences in thinking behavior between species and between individuals within a species, particularly the human species. These relate to the complexity of the reaction tendencies that may be elicited in our Stage II, and to the number of additional reaction tendencies that can be evoked if Stage III has not resulted in solution.

As we have indicated, reaction tendencies may range from unlearned responses to principles or concepts of any degree of sophistication. The repertory of appropriate reaction tendencies is believed to be a primary determiner of the efficiency of thinking and, if this is true, their nature, development, organization, characteristics, and mode of operation in thinking or problem solution become a primary concern. Our first interest is, then, the experimental data bearing on organized reaction tendencies variously described as principles, concepts, learning sets, or hypotheses.

Organized Response Patterns

Whatever differences modern authorities may hold concerning the nature of thinking, most, if not all, agree that thinking is not a random trial-and-error process but operates in large part through the elicitation and selection of appropriate responses, many of which may be highly organized response patterns. Various names have been given to designate these response patterns, but the term most frequently used is that of "concept." This term has been used in a restricted sense to refer only to a symbolic response to related stimulus patterns such as in Smoke's definition (1932). The term concept has also been used more broadly to refer to any principle or rule that may operate in the solution of a puzzle. Dewey's hypothesis would appear to be the organized response patterns selected for trial during thinking activity, and Krechevsky utilized the term in a similar way in the description of his animal studies. Maier speaks of "habitual direction in thinking" which appears to play a similar role. Katona's "organized wholes" and Wertheimer's "structural features" and "sensible hypotheses" also describe organized response patterns utilizable in the solution of complex and relatively new problems.

In describing the formation of organized response patterns the present author will employ the terms chosen by the original authors to describe the organized response patterns they chose to study: concept, learning set, hypothesis, principle, or structural feature.

Review of the literature suggests that a certain uniformity exists in this field. The studies consistently show that organized response patterns only arise after learning (or, at least, opportunity for learning) and usually only after prolonged learning. Furthermore, the studies attest to the fact that broad concepts or principles do not generalize spontaneously from learning or overlearning any specific problem; breadth of concept is obtained from training in a wide variety of situations. Breadth, rather than intensity of training, appears to be the key to efficient concept formation whether we deal with subhuman or with human subjects.

Formation of Organized Response Patterns

The construct of generalization has been so often stressed and strongly emphasized that one might well believe from the theoretical literature that solution of a single problem representative of some class would lead to wide generalization to similar problems. The experimental literature by no means supports such an assumption, however, whether we study subhuman or human subjects. Rats, monkeys, children, and human adults form organized response tendencies—concepts, learning sets, principles, or hypotheses—that can subsequently be used in the solution of new or relatively new problems, but such useful and usable responses are formed only after the organism has had breadth of experience in solving many similar problems. This interproblem generalization doubtless becomes greater as we ascend the phyletic series but the fundamental principle is true throughout. Unfortunately there have been few studies designed to test this particular thesis, and those studies which exist are not entirely comparable either in problem or in method.

In Fields' studies (1932, 1935) on the formation of the concept of triangularity in the rat, for example, very limited generalization was obtained following initial training in discrimination of a white triangle and a white circle on a black ground. Failure was encountered when even relatively slight changes were made in the stimulus situation such as changing the background, rotating the positive stimulus, or substituting a new negative stimulus for the familiar one. Yet training on a wide variety of triangularity situations enables the subjects later to respond effectively in all of these situations regarded as tests of a concept of triangularity. The experiments unfortunately were not designed to indicate the extent to which the performance following the varied training resulted from specific additional training or generalized from the broader type of training.

A different approach to the problem of the formation of organized response patterns was carried out by Harlow (1950a), who trained eight monkeys on a long series of problems of comparable difficulty and studied both intraproblem and interproblem learning. Over three hundred successive six-trial object discrimination problems were

presented and each problem utilized a different pair of stimuli. Little learning appeared in the early problems, and such improvement as did occur was slow and gradual. But efficiency of learning increased continually until the monkeys' performance on all problems approached theoretical perfection. Thus, if the monkey chose correctly on the first trial of a problem, he rarely made an error on a

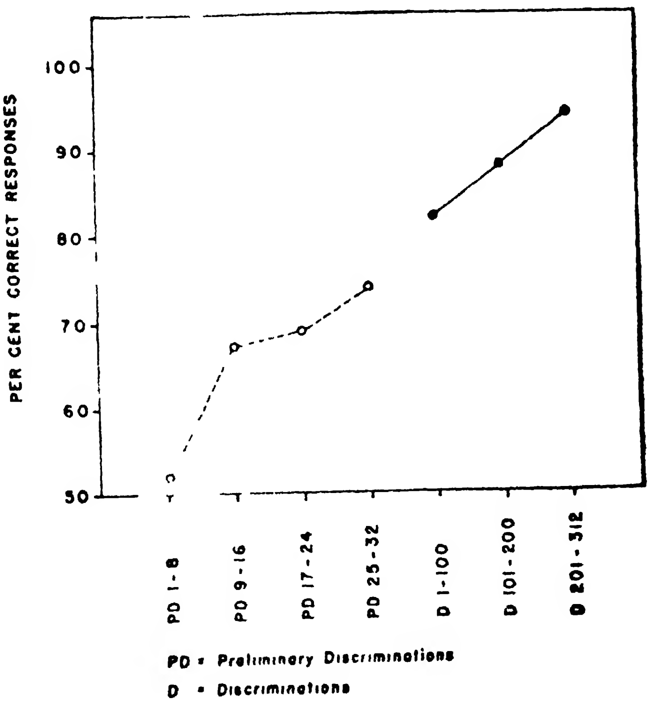

FIG. 10.3. Discrimination learning set curve based on per cent correct Trial 2 responses on successive blocks of problems. (From H. F. Harlow, *Psychol. Rev.*, 1949, 56, 51-65, by permission of the American Psychological Association.)

succeeding trial, and if by chance he erred on the first trial, he immediately shifted to the correct object and continued henceforth to choose it.

Interproblem improvement, which Harlow has referred to as the formation of a learning set, is illustrated in Figure 10.3, which shows the percentage of correct Trial 2 responses in 344 successive problems. These data show that transfer of learning from problem

to problem is as orderly as improvement from trial to trial within problems. Of particular theoretical import is the finding that a kind of problem which the monkeys originally solved by trial and error came later to be solved by immediate or insight learning, an ability that was acquired in a highly orderly and predictable manner. The monkey now possessed a concept or principle which he could employ in a variety of new situations, thereby greatly simplifying the solution of many problems.

In subsequent studies other more complicated learning problems were attacked and yielded parallel results. Learning of the early problems in each series was slow and laborious, but as the series pro-

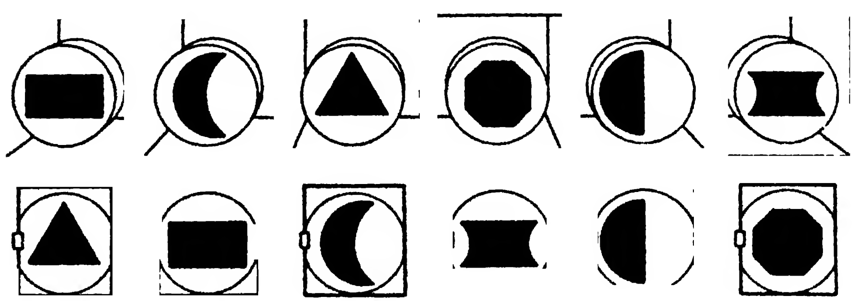

FIG. 10.4. Sample form-matching situation used by Roberts. (From K E Roberts, *Univ Iowa Stud Child Welfare*, 1933, 7, No 3, by permission of the author and the Editor of University of Iowa Studies in Child Welfare)

gressed, learning became gradually more efficient until the stage was reached in which the subjects showed almost errorless performance on new problems Furthermore, evidence was found that learning sets might operate as units which could be elicited with far greater than chance efficiency as the demands of an experimental situation changed in complicated ways without warning.

An ingenious experiment by Roberts (1933) illustrates the development and utilization of an organized response pattern in a series of problems systematically varied in similarity. Three groups of learning situations consisting of three color, three form, and three size matching problems were presented in balanced order to 40 children three to seven years of age. The apparatus, a two-story six-compartment multiple-choice box, is illustrated in Figure 10.4. The

upper sections have windows behind which stimulus objects are displayed, and the lower parts, doors behind which are cards with the matching stimuli. On each trial only one of the six pairs of stimuli matched. The color situations included (1) six airplanes of one color, six doors of different colors, one of which matched the airplanes; (2) six airplanes of different colors, one of which matched

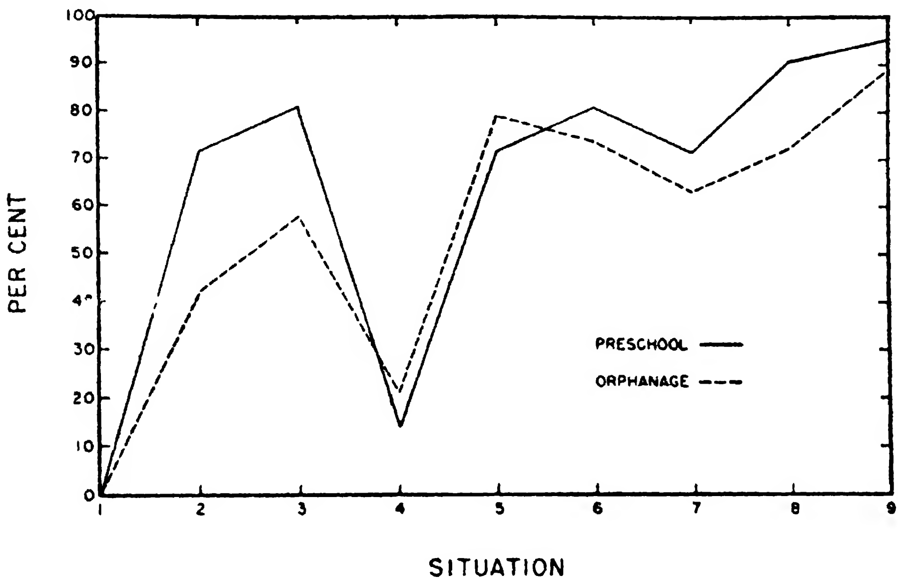

Fig. 10.5 Percentage of subjects solving successive matching problems without error (From K. E. Roberts, *Univ. Iowa Stud. Child Welfare*, 1933, 7, No. 3, by permission of the author and the Editor of University of Iowa Studies in Child Welfare.)

the uniformly colored doors; (3) six varicolored airplanes and six varicolored doors so arranged that only one combination matched. The three form situations followed the same general plan in the utilization of six different forms pasted on boxes for the upper parts of the compartments and on cards for the lower parts. Similarly, the size situations utilized circles of different sizes pasted on boxes for the upper sections and on cards for the lower sections.

The design permits study of transfer under conditions of slight change of situation within each group of problems and marked change of situation between problem groups. Figure 10.5 shows the

results in terms of errorless transfer for combined subject groups. *It is seen that transfer is high within groups and consistently drops on the first situation in a new problem group.* Furthermore, a cumulative transfer effect is apparent not only within groups of problems, but also between groups of problems if one compares the corresponding situations of the three groups. These findings demonstrate for children the slow but cumulative transfer effect of learning in multiple similar situations.

The implication of the preceding studies is that efficient organized response patterns are only formed, or, at least, only formed efficiently, after the organism has had experience on a series of related problems. Training on a single problem, although intensive, does not provide for effective transfer to a variety of similar problems. The experiments cited, however, have not been designed to test this point. A direct test of the relative efficiency for transfer of narrow and broad training is provided by a pair of studies of learning by monkeys of the oddity principle problem (Meyer and Harlow, 1949; Harlow, Meyer, and Settlage, 1951).

The oddity principle problem requires selection of the odd member of three stimuli, even though the particular stimulus odd on one trial is doubly represented on half of the other trials. Under one condition of training, 24 trials a day were given on a single set of two pairs of stimuli until mastery. Under the other condition, a new set of stimuli was presented each day and training continued, 24 trials per day, for 60 days. Under both conditions monkeys required about 750 trials to master the oddity principle problem, but transfer tests on the subjects trained in the single situation revealed extremely inefficient performance contrasting sharply with the near-perfect performance of the monkeys trained by the multiple-situation method. Thus, monkeys trained in the multiple situations learned to solve *any* oddity problem in the same number of trials it took monkeys with specific training to master only one problem.

The literature clearly indicates that this general principle presented for subhuman subjects and children applies also to adult man. It may be noted in this regard that Hull (1920), in his classical study of concept formation, reported improvement in rate of concept

formation from problem to problem. Furthermore, subjects trained from simple to increasingly complex characters did no better than subjects trained from complex to simpler characters. Thus, if difficulty in context operated as a factor, it was completely obscured by the dominant variable of interproblem learning. Good evidence for this principle of operation is seen also in the descriptive reports and experimental studies by members of the Gestalt School (Katona, 1940; Wertheimer, 1943), although their interpretations differ from those given in this paper.

Illustrative is a group of experiments reported by Katona (1940) involving solution of match problems that require increasing and decreasing the number of squares in a design by moving limited numbers of match elements in prescribed ways. One investigation utilized four college undergraduate groups of approximately 25 subjects each. Group Mem (Memorizing) was presented specific solutions of a number of problems; Group Help had the organization of the successive steps "presented in a structurally consistent way" for a number of situations; Group Arith (Arithmetic) was taught a principle and shown how to apply it; and Group Con (Control) had no instruction. Retention tests of the training problems and transfer tests on new problems showed Group Help to be most efficient, Group Arith second, Group Mem third, and Group Con fourth. In addition to different instructions and illustrations for the groups, differential opportunities were provided for visual exploration by leaving the illustrations on the blackboard for Group Help throughout the training and erasing each illustration after presentation to Group Mem.

That the subjects trained by specific procedures performed less adequately than subjects trained to search for and understand the problem solutions through step-by-step illustrations and explanations is not surprising. An important result, however, in spite of the limitations of the experimental design, is the finding that the subjects given the verbalized principle did less well than those taught to search for solutions in a number of problems. Although learning through knowledge of the principle was superior to specific training or no training, its efficiency was clearly limited.

Another method of intermediate efficiency described in one of the less adequately controlled experiments was the teaching of the principle involved in the match puzzles in terms of the *creation of holes* and the *loosening of the figure*. Like verbal training, this method was superior to the obviously ineffective technique of teaching specific solutions, but was less effective than either unassisted solution of a variety of match tasks or learning from a variety of examples.

There is a tendency for Katona to describe any successful method as a method which involves "formulated structural principles" and "understanding." There appears, however, to be one principle common to the efficient methods—training or instruction in multiple different situations

It is, of course, extremely difficult to design precise and specific studies of the formation of organized response patterns in the human being because of the unknown past experiences of the subjects, the variability of these past experiences, and the very wealth of previously established concepts Katona's studies are of extreme methodological importance and suggest approaches to this problem that might profitably be followed on both young and adult human beings

Factors Influencing the Formation of Organized Response Patterns

Since the utilization of organized response patterns is so important in the solution of complex problems, it is essential to consider the various factors that operate to impose quantitative or qualitative limitations upon the formation of such mechanisms.

Variables Relating to the Organism.—Interspecies and intraspecies comparisons always present difficulties of interpretation because of the problem of obtaining comparable training and motivational procedures while simultaneously controlling the demands made on differential sensory and motor abilities of the subjects. In spite of such difficulties some generalizations appear secure. Phyletic position appears to be directly related to both ease of formation of organized response patterns and the complexity of the patterns that can be acquired. Comparatively young human children master

principles involved in the efficient use of tools with a speed and breadth not found in the mature chimpanzee or monkey. Gellerman found that children displayed far greater proficiency and breadth in the mastery of the concept of triangularity than did his chimpanzees (1933a, 1933b). Harlow and Harlow (1949) trained monkeys and children in parallel situations demanding responses ordinarily practiced by both in natural living conditions (visual discrimination and reaching) and demonstrated the superiority of preschool children over pubescent and adult monkeys in discrimination learning set formation and discrimination-reversal set formation.

A wealth of experimental studies carried out by Nissen and associates on chimpanzees, and Harlow and associates on monkeys indicate that these subhuman primates can form more complexly organized response patterns than can any other order of mammals Unfortunately, the rat is the only other mammal intensively studied, but scattered investigations on the dog, cat, and raccoon, along with the rat studies, lend support to this thesis.

Intraspecies comparisons made on man support the hypothesis that age from birth to maturity is positively related to increased ability to form multiple and complex concepts The accumulated data of the mental tests give results in keeping with this assumption, and such laboratory studies as have been carried out, concur. Thus Welch and Long (1940a, 1940b) have reported a series of genetic studies of concept formation in which added complexities in the problem situation generally raise the minimal age for problem solution. Mental age tends to be more closely related than chronological age to ease of formation of organized response patterns (Roberts, 1933; Kuenne, 1946)

With advancing years there is evidence to suggest that the ability to form and recombine organized response patterns decreases. Thus Lehman's statistical studies indicate that renowned men of science and the humanities contributed their first important creative work before the age of 25 years and that the modal score for peak performance is below 35 years of age 1945, 1946) Bjorksten (1946) has criticized Lehman's work on the grounds that the scientist has less time for creative work with advancing years because of the pres-

sure of other obligations. Other data relating to pathological aging are given below.

A current research interest is the relation of behavior pathology to the formation of concepts. A technique frequently used for such clinical investigation is the Vigotsky Test, which requires the classification of multivariant blocks into four categories: large-tall, large-flat, small-tall, and small-flat In a study by Hanfmann and Kasanin (1942) the test was administered to 50 highly selected college graduates and 12 schizophrenic patients with at least some college education, and 45 normal subjects, 50 schizophrenics, and 24 elderly organic patients of somewhat comparable educational and occupational level. A highly significant difference was obtained between the performance of the very superior normal subjects and all other groups. The normal subjects of average education performed at a level significantly beyond that of the comparable schizophrenics and the elderly organic patients The mean scores of the normal subjects with average education were similar to those of the schizophrenics with superior education, but the range of performance within the schizophrenic group was very much greater

The data strongly suggest that schizophrenia impairs concept formation or, at least, greatly increases the variability of performance on such tasks. Severe and chronic destructive brain diseases operating in aged patients of limited abilties and probably long removed from testing experiences leave the individuals with limited ability to form complex concepts in an emotionally demanding situation.

Cameron (1939), in comparing the Vigotsky test performance of five severely disorganized schizophrenic patients and six seriously deteriorated seniles, was impressed by the failure of the schizophrenic patients to limit themselves to the arbitrary task The immediate perceptual field, the patient's thoughts about absent things and events, and material derived from personal preoccupations were persistently introduced into the problem. By contrast the seniles restricted themselves to the arbitrary limits imposed by the experimental set-up, but they failed the test because they clung monotonously to one or two prevailing principles.

Studies, of which those cited are representative, clearly show that there are important interspecies and intraspecies variables which operate to influence the efficiency of formation of organized response patterns and, in turn, limit the number and generality of the attained concepts and principles Little is known, however, of the nature of the limiting factors and their manner of operation

Variables Relating to the General Situation.—It has been implicitly accepted that the general experimental situation affects the ease of formation of organized response patterns, and this assumption appears to be validated indirectly by the findings of a number of different studies varying primarily in the nature of the instructions provided the subjects A few studies have been deliberately designed to control all the variables except those relating to instructions, and a few have attacked the situational variables of number of contexts in which a concept appears or general method of attack

One study bearing on instructions raises as many questions as it answers Green (1943) conducted a modification of the Roberts experiment, varying the original study by providing two types of tuition either prior to or following learning of the first situation Subjects were preschool and primary children The significant finding was that initial tuition accelerated learning but either failed to aid or actually reduced transfer to the new situation These results suggest the need for further investigation of the conditions of instruction that facilitate learning for transfer as opposed to facilitation of learning merely in the specific situation

A detailed study of the effects of sets induced by verbal instructions on the efficiency of concept formation was carried out by Reed (1946a) The materials were 42 cards on whose face were four English words, one of which was a key to a category symbolized by a nonsense syllable The key words fell into six classes having seven members each An anticipation method was used in which the subject had three seconds to name a card without prompting and two additional seconds to look at the card and, if prompting had occurred to repeat the name The 30 subjects of Group 4 were instructed *to learn concepts*, and the 21 subjects in Group 5 were instructed *to learn the name of each card*

The group instructed merely to learn names took about 33 per cent more prompts to learn and approximately 50 per cent more prompts to relearn than the group given concept formation instructions. Qualitative differences also appeared. Group 4, as compared with 5, formed a greater percentage of *consistent concepts,* indicated by ability to name the class or the key words, and fewer *inconsistent concepts,* indicated by association of a syllable with the appropriate cards but without ability to name the class or the correct key words.

Using the same technique as for Group 4, Reed (1946b) subsequently measured the influence of length of series on learning and retention of concepts. He tested the 21 subjects of Group 6 on 24 cards containing four instances of each concept, and the 21 subjects of Group 7 on 60 cards having 10 instances of each concept. The data of Group 4 of the previous experiment are used for the 42-card intermediate series. The ratio between number of prompts required for mastery and length of series decreased regularly with increase in series length. Increasing the number of instances also gave increasing frequencies of consistent and decreasing frequencies of inconsistent concepts. Reed concludes that "The greater the number of new situations in which a certain constant or concept appears, the more likely it is to be observed and the more precisely will it be defined" (p. 170).

Reed's term "inconsistent concepts" merits consideration. Many of our concepts are completely arbitrary and determined by fiat. Thus many of the wide number of objects which we may classify as steel have little or no perceivable similarity. They are differently shaped, differently colored, and differently weighted. We react to them as a class simply because we have been trained to accept them arbitrarily as belonging to a class. In the same way we come to classify whales and dolphins as "mammals," lemurs as "primates," and mushrooms as "fungi."

Experiments carried out by Heidbreder and associates, described in detail in the next section of this chapter, further illustrate the importance of the general situation in concept formation. The studies utilized two different types of procedures, one involving modified associative learning and the other, sorting, matching, and classifi-

catory methods. The results indicate much more rapid concept formation in the second type of experiment than in the first. The two procedures also gave differential results on the relative difficulty of concepts based on varying kinds of stimulus materials.

Variables Relating to the Stimuli.—The bulk of experimental work on factors determining the formation of organized response patterns has been devoted to the investigation of the effects of the stimulus characteristics, and the data clearly indicate that efficiency of concept formation is related to and dependent on the efficiency of perception of the basic data. There are practical implications from the results of these experiments. Difficulties of concept formation and subsequent thinking may result in part from the perceptual difficulties inherent in the conventional symbols as well as from "intellectual" limitations of the subjects.

The relative difficulty of responses to color and form has been extensively investigated in monkeys and chimpanzees. Harlow (1945) reported that monkeys experienced on discriminations involving multivariant stimulus objects performed with significantly more efficiency on discriminations between stimuli differing only in color than on stimuli differing only in form. The color attribute was also found to be more effective than the form attribute of the same stimulus objects for monkeys learning the Weigl oddity principle problem (Young and Harlow, 1943). Transfer of the oddity principle learned on multivariant stimuli to new stimuli differing only in one dimension has been found to be more efficient to color than to either form or size differences (Meyer and Harlow, 1949). Similar results have also been reported for the chimpanzee by Nissen and McCulloch (1937).

The relative potency of color and form in the perception of human subjects, especially children, has been extensively investigated, although the function of color-form dominance in concept formation has been studied infrequently. Presumably, however, ease of formation of concepts based on color or form is directly related to perceptual potency. The perceptual studies have utilized both matching and conditioning techniques with ambiguous color-form stimuli and have reported conflicting findings. Brian and Goodenough

(1929), for example, reported in an experimental study requiring matching of six forms and six colors that form is potent in the early preschool years, color in the late preschool years, and form thereafter. Youngs (1949) found in a conditioning study using four colors and four forms that form was overwhelmingly dominant throughout the preschool age range. The differences are probably reconcilable in terms of the degree of difference of the forms used in the two experiments. The first investigators actually used only three independent forms, but varied the position of these forms to obtain six "forms," whereas the later investigator used novel, markedly different forms. This interpretation is consistent with the findings of Huang (1945) that relative potency of color and form in kindergarten children is directly related to the degree of difference of the stimuli in color and form attributes.

The most intensive investigations of stimulus variables in concept formation are those of Heidbreder and associates studying systematically the relative speed of formation of color, form, object, and number concepts by several hundred college students. The early studies were ostensibly memory experiments in which the experimenter pronounced a different nonsense syllable name and the subject repeated it for each of a series of drawings exposed by a revolving drum apparatus. Sixteen different series of nine drawings each were used. Each series contained nine different kinds of drawings, and each kind was included in all 16 series (see Figure 10.6). The same nonsense syllable was applied to all drawings of a kind. After the first presentation of the initial series, the subject was given two seconds to anticipate the name of each drawing. The first series was repeated until the subject could name all the drawings in two consecutive presentations. Subsequent series used the anticipation method throughout, and each series was presented through two correct consecutive namings. A concept was considered to be attained (CA) when the subject could provide the name of a kind of drawing on its initial presentation in a series and all subsequent series.

In the first three experiments, A, B, and C (1947), the concepts were grouped equally into the three categories of concrete objects, spatial forms, and numbers, and they were regularly *attained* in this

order. Furthermore, the differences in points of attainment of the various categories tended to be statistically significant and remarkably constant from subject to subject. Heidbreder accounts for these results in terms of the degree of thing-character of the materials, i.e., the degree to which the materials resemble concrete objects.

Similar results were obtained in the next two studies (Heidbreder, Bensley, and Ivy, 1948) in which special attention was given to equalizing the situational features determining responses. The new category of color was utilized in these studies. In accord with

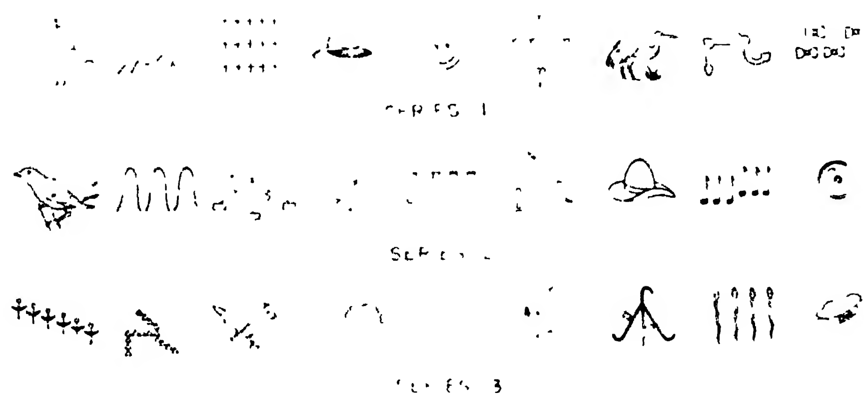

FIG. 106 Sample series of drawings used by Heidbreder in Experiments B and C. (Redrawn from E. Heidbreder, *J. Psychol.* 1947, 24, 93-138, by permission of the author and the Editor of the Journal of Psychology.)

the hypothesis that color is more thing-like than number but less thing-like than form, color concepts were attained at an intermediate rate, although some of the differences were not significant. Two more experiments (Heidbreder and Overstreet, 1948), confined to object, color, and form concepts and, varying the contexts, yielded results in conflict with this hypothesis in that color was attained at a rate not significantly different from that for either of the other categories.

In two later papers Heidbreder reports the results from various sorting, matching, and classificatory studies. The major experiment in the first of these papers (1948) required sorting the 144 drawings of B and C, now reproduced on small cards, into nine piles and, if

desired, a discard pile. Only 4 of the 18 subjects classified the cards according to the intended concepts. The group achieved high success on object classes, moderate success on one form class, and low success on the remaining concepts. Only one form class gave better results than were obtained for number. Heidbreder partly accounts for the differences between the results of this study and the modified memory experiments in terms of the increased situational support offered by the more perceptual, more manual technique, and subsequently varied the situational supports in a series of 11 sorting experiments (1949a). By emphasizing particular attributes of the stimuli she obtained preferential sorting for object, form, and color, and by using postage stamps as stimuli, obtained preferential sorting for number.

The last (1949b) of Heidbreder's experiments on concept formation substituted brief verbal phrases for the drawings of the previous studies. A modified memory procedure was used for 40 subjects and a card-sorting technique for 36 Both groups attained concepts of concrete objects first, numbers next, and spatial forms last, an order "*not* correlated with the degree of thing-character attributable to the critical features of their instances. Instead, the obtained order was positively correlated with the degree of efficiency with which verbal phrases performed their *semantic* function—specifically with the *explicitness* with which they referred to critical features and with the *directive character* of their syntactic forms" (p. 306).

Thus, the Heidbreder studies indicate the complexity of the operation of stimulus variables in concept formation Changes in the context in which the types of concepts appear, changes in the type of response required of the subject, and changes in the concreteness of the materials have been demonstrated to alter the difficulty value of the same categories of concepts. The entire series of studies raises many questions that might well be pursued by the workers who would integrate into a general theory the research findings on concept formation.

Numerous other studies investigating the effects on concept formation of varying the stimulus characteristics might profitably be

referred to by the interested reader. Especially noteworthy are the genetic studies of Welch and Long (1940a, 1940b) and Long and Welch (1941) which are too lengthy to be described in this chapter.

Elicitation of Habits and Organized Response Patterns

The fact that an individual has previously acquired either habits or organized response patterns that can lead to effective solution of a particular problem is not, of course, sufficient to insure that he will be able to utilize these response patterns when faced with the problem. Failure may result from a multitude of inadequacies. The stimulus situation may be too vague to call forth any habits or response patterns, or those elicited may be irrelevant. The stimulus situation may be clear, but, for various reasons, the appropriate habits or response patterns are not immediately available. The responses that *are* evoked are inadequate for problem solution. If the task involves the solution of a series of problems, as in the shift of set on "abstract attitude" problems, there may be elicited an organized response pattern that will solve the first problem of the series, but the subject will be unable to go farther.

The elicitation of organized response patterns in experimental studies has been approached under two different conditions. One condition, which might be called *situational elicitation*, provides the cues to elicitation through spatially or serially presented concrete stimuli. The other condition, *symbolic elicitation*, provides symbolic cues (alone or in association with concrete stimulus cues) that elicit the particular concepts or principles. These response patterns have become associated with the symbols through past experience and may serve directly in the solution of the problem or, in turn, elicit other organized response patterns that may lead to solution. The symbols provided human subjects are typically, but not always, linguistic, whereas nonlinguistic symbols are used for subhuman animals.

Interest in this field stems from the pioneer researches of Ach and the subsequent elaborations of these studies into tests for comparing the behavior of normal and brain-injured persons. Basic

designs for the clinical elicitation experiments originated in the work of Gelb and Goldstein (1925) and Weigl (1941).

When presented with a problem, subhuman and human animals typically do something, inappropriate though it may be. After preliminary straight-away training, rats placed in a maze will run, and into culs-de-sac as well as true paths. Sophisticated monkeys and chimpanzees placed in a discrimination problem situation engage in behavior, random or organized, appropriate or inappropriate. Amazing accounts of this elicitation of *something* in imbeciles faced with problems beyond their ability have been given by Wembridge (1931). *Skill* may be defined as "skull" or "you fry it" to the satisfaction of the testee if not the tester, and the question, "If two apples cost 5 cents, how much do 12 apples cost?" elicits "arithmetic" even though the processes have little or no bearing on the question asked. Nor is such behavior limited to mental defectives, as is indicated by Thorndike's report (1949) of definitions written by college graduates, graduate students, and summer school students Members of this select group, for example, defined *cacophony* as "a substitute for cocoa," *carmagnole*, "a car for magnolias," and *abattoir*, "ability to bat." As long as the animal is motivated to perform in a situation "something" will be elicited.

But the interest in elicitation in problem solution relates not only to the factors that operate to evoke successful and unsuccessful responses, but also to those that operate in eliciting multiple attacks should the initial approach prove unsatisfactory to the tested animal. Dissatisfaction, it should be noted, may result from failure to obtain the incentive or from stresses imposed by the experimenter.

Observational and introspective evidence has led some authorities (Goldstein and Scheerer, 1941; Weigl, 1941) to advocate that the ability to respond with a variety of appropriate organized response patterns to the same stimulus situation is qualitatively different from ability to respond with only one pattern The assumption is that shifting from one aspect of an object or situation to other aspects depends upon a "categorical attitude" as opposed to a "concrete attitude," and is absent in subhuman animals, young children, and brain-damaged human adults. It is interesting in the light of this hypothesis to examine representative experimental results.

Situational Elicitation.—Repetitive training on any task in which the stimuli remain the same while the situational cues change may be used to measure the ability of animals to respond with alternate learned habits or organized response patterns. One of the simplest of these is the discrimination reversal problem in which, for a number of trials, stimulus "A" is correct after which stimulus "B" becomes correct for a number of trials.

A pioneer research on this problem was carried out by Fritz (1931), who trained rats for almost 5000 trials on a series of fre-

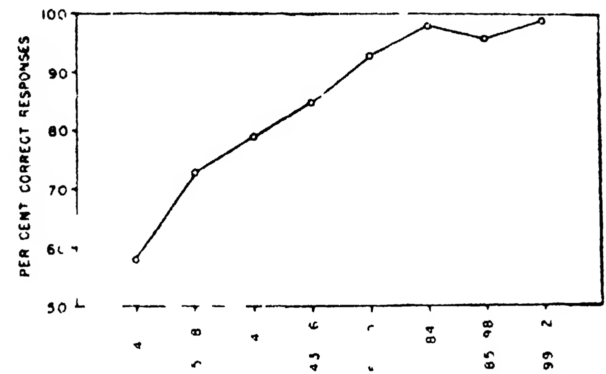

Fig. 107. Discrimination reversal learning set curve based on per cent correct Reversal Trial 2 responses on successive blocks of 14 problems. (From H. F. Harlow, *J. comp. physiol. Psychol.*, 1950, 43, 231-239, by permission of the American Psychological Association.)

quently reversing black-white discrimination problems. Since Fritz obtained no indication of improvement from reversal to reversal there is no evidence either for the formation or the elicitation of appropriate learning sets.

A series of 112 discrimination reversal problems was given by Harlow (1950b) to a group of eight monkeys that had previously mastered the discrimination task. The procedure was to run the monkeys on a series of discrimination problems for 7, 9, or 11 trials and then to reverse the reward value of the stimuli for 8 trials. Improvement in performance was gradual and by the seventieth problem, the monkeys were making almost perfect scores on and after Reversal Trial 2, the first trial following the "informing" initial trial reversing the reward value of the stimuli (see Figure 107).

Thus it is apparent that the monkeys had formed an organized pattern of discrimination reversal that could usually be elicited and maintained with maximum efficiency.

The elicitation of organized response patterns in a more complex situation was investigated by Zable and Harlow (1946) in normal monkeys and by Settlage, Zable and Harlow (1948) in both normal monkeys and monkeys with bilateral destruction of the prefrontal areas of the cortex. The tasks, identical in both experiments, involved training and testing on antagonistic positional and object-quality discriminations In the final stage of the studies, the monkeys were given 60 trials a day on a single pair of objects. For 15 trials Object A was correct, for 15 trials Object B was correct, for 15 trials the Right position was correct, and for 15 trials the Left position was correct. No cue was given for problem shifts, and the order in which the four different parts of the problem were presented changed from day to day in an irregular predetermined manner for the 30 days of the experiment.

The normal monkeys demonstrated good ability to respond with and maintain the correct organized response patterns They averaged only 3.7 errors per sub-problem (an average of 1.5 "essential errors" is needed to inform the animal of the change in problem) and made approximately one error in the last 10 trials of each sub-problem. The object-quality discriminations were made with greater efficiency than the positional discriminations, data in keeping with the previous training histories of the subjects. The operated monkeys were significantly inferior to the normal monkeys, averaging nearly 6 errors per sub-problem. There is, however, no evidence of qualitative differences between the groups.

The problem of the elicitation of organized response patterns has been investigated in human subjects with the same basic technique of shifting the problem without warning or instruction. In the Wisconsin Card Sorting Test (WCST) the subjects are required to sort a pack of 64 cards arranged in standard order and picturing one to four identical figures of a single color. Four different forms and four colors are used, and the cards are sorted into four piles in front of four multivariant sample cards.

The initial study by Berg (1948) measured the performance of 51 college students. Predetermined cycles of color, number, and form categories were presented until nine categories (three of each kind) had been completed. As the subject sorted each card he was informed of his correctness. After five successive correct placements, the "correct" category was shifted. On the basis of performance and verbal reports given at the end of the experiment, the subjects were classified into Group A, subjects who discovered the shifting of categories; Group B, those who had a vague notion that the experimenter was shifting; and Group C, those who had no notion that there was a shift. Group A averaged approximately 3 errors per category, Group B approximately 8 errors, and Group C approximately 15 errors on the last six categories. The minimal average number of errors required by this experiment, 1.5, was not attained by any group and not approached by any group other than the A Group.

Significant differences in performance on the WCST were reported by Tobey (1949) between young normal adults and schizophrenic patients matched for age, IQ scores and educational achievement. The task required sorting, in order, for color, form, number, color, form, and number to the criterion of 10 consecutive correct matches for each category. Sixteen of the 22 schizophrenic subjects and eight of the 47 normal subjects failed to complete the test, a difference significant at the 1 per cent confidence level. The patients who completed the test were less efficient than the normal subjects both in terms of errors and number of trials. Furthermore, the patients had more difficulty in maintaining a correct category once it was achieved, as indicated by a greater number of runs of three to nine correct responses. It should be noted that the differences between the schizophrenic and normal subjects were only quantitative; subjects of both groups taken together varied widely in the various measures of performance, and normal subjects were found at both extremes.

Difficulty in responding with alternate organized response processes was described by Weigl (1941) in patients with cerebral lesions, and this difficulty contrasted with the efficient performance of normal adults. One of the test situations presented 12 cardboard figures representing four color and three form categories. The "typical"

patient sorted for color and had great difficulty sorting for form even when such sorting was demonstrated. In another sorting test the subjects were presented with 30 objects readily classifiable in terms of use, color, form, and double occurrence. The "typical" patient classified according to use and denied other possible arrangements. For most normal adult subjects "points of shift" from one category to another appeared almost constantly.

Another investigation of the elicitation of organized response patterns was made by Bolles (1937), who studied sorting and matching performances of nine "chronic" schizophrenics, ten institutionalized aments, and ten normal children. The mental ages of the groups approximated nine years. In one situation the subjects were presented with three wool skeins and asked to which of the other two—one equivalent in brightness, the other equivalent in hue - the center sample belonged. Nine children matched for hue, six dements chose brightness, and five aments stated that the sample belonged to neither!

Using a slight modification of the Weigl form-color sorting test, Bolles found that one dement, two aments, and all ten children grouped according to two categories. Comparable results were obtained on a sorting test consisting of 38 figures that varied in four attributes. On a sorting test involving 31 miscellaneous objects which could be arranged in color, material, form, double occurrence, and use categories, nine of the ten children spontaneously sorted for material or form (as well as use), whereas none of the aments and only one of the schizophrenic group did so. When the sortings were done by the examiner seven, eight, and nine children correctly interpreted form, material, and color, respectively. One ament successfully interpreted the color sorting, and one dement, the material and form categories. Double occurrence was neither correctly interpreted nor spontaneously grouped by any of the subjects.

These results clearly show the marked superiority of the normal nine-year-old child over *chronic* schizophrenics and *institutionalized* low morons in the elicitation of organized response patterns. Even so, it should be noted that there is some overlap in performance—a

child occasionally failed on some test solved by a member of one of the clinical groups.

An interesting variation of the Weigl test method has recently been reported by Scheerer (1949). One phase of the experiment used 15 different forms of three basic kinds, all colored yellow, and in a second phase the 15 forms were represented in each of three colors—

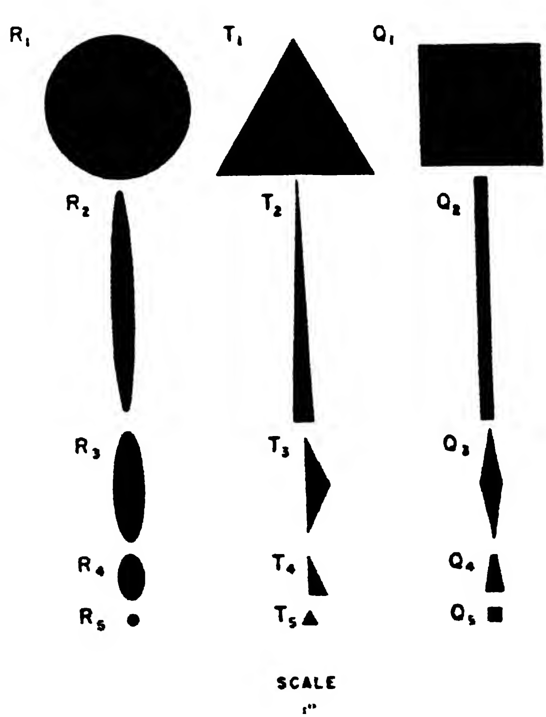

Fig. 10.8 Forms used by Scheerer. (From M. Scheerer, *Confinia Neurologica* 1949, 9, 232-254, by permission of the author and the Editor of Confinia Neurologica.)

red, green, and yellow. Figure 10.8 shows the forms. Four groups of subjects were tested: (I) 44 college students; (II) and (III) normal and brain-injured groups of 20 matched for age (means: 34, 39) and IQ (means: 96, 93); and (IV) a retarded high school group of 20 subjects, 14-16 years of age (mean IQ 76).

In the first phase of the experiment the subjects were asked to sort each of the 12 small figures separately presented, in relation to

three large figures simultaneously presented. If failure resulted, th following helps (additional cues to elicitation) were given (p. 239)

"(1) Can it go elsewhere?
"(2) Could it be placed here too .. could it belong here too?
"(3) What is this figure ... what shape has it? How many sides (or corners) has this one [test figure]? How many sides (or corners) has this one [model figure]?"

The critical figures were Q_3 and Q_4 which, though quadrilatera are easily categorized as triangles because of their "pointedness The per cent correct responses made by the four groups on thes critical figures are given below:

TABLE 10.1

Groups	Per Cent with Initial Success	Per Cent Failures Succeeding on 1st Help	2nd Help	3rd Help
College	30	77	23	--
Normal, noncollege	10	66	17	17
Brain-injured	0	0	0	25
Retarded	20	81	19	

The almost total inability of the brain-injured subjects to profit from helps—to elicit appropriate organized response patterns to highly appropriate verbal cues is the most striking fact illustrated by the table Another result also deserves consideration The retarded 14- to 16-year-olds were *superior to the normal 34-year-olds on every measure* despite a 20-point inferiority in mean IQ score. Whether this is a matter of age or time since formal education is unknown, although the retarded subjects who had recently had geometry did no better than the others

In the second phase of the experiment the subjects were required to sort the 45 figures without the aid of models or verbal cues. All of the college students, 75 per cent of the noncollege adult group, 10 per cent of the brain-injured, and 30 per cent of the retarded successfully sorted. The only striking deviations of these data from those obtained in the first phase of the experiment are the marked

superiority of the noncollege adults over the retarded group and the failure of the retarded group to do significantly better than the brain-injured.

Although the studies illustrate that the possession of organized response patterns is no guarantee of ability to use them, few have been directly concerned with the mechanisms of elicitation or the factors that affect their operation. The investigations cited indicate that age, experience, intelligence, psychosis, and cortical integrity operate as influencing factors. How they operate, however, is unknown. Various hypothetical mechanisms have been suggested to account for failures of elicitation, including perseveration, concrete attitude, rigidity, and emotional blocks, but the field remains open for exploration and the formulation of more specific theories.

Symbolic Elicitation.—Both sign and symbol have been defined in diverse ways. To facilitate discussion, we arbitrarily designate a *sign* as a stimulus standing for a particular object or response, and a *symbol* as a stimulus standing for a category or group of objects or actions which we have been referring to as an organized response pattern. Words may be either signs or symbols, serving either as stimuli eliciting specific responses or highly organized response patterns. For a single individual the same word may on one occasion operate as a sign and on another occasion as a symbol; the genetically underprivileged individual more frequently uses words as signs, the genetically blessed, as symbols. Since human symbols are so frequently linguistic in nature, the importance of nonlinguistic symbols in man may be ignored and their use by subhuman animals denied. But recent studies of subhuman primate behavior, particularly those by Weinstein (1945), Simpson and Harlow (1944), and Nissen (1951) present convincing evidence of subhuman symbolic behavior.

One of the primary functions of symbols, linguistic or nonlinguistic, is that of *facilitating the elicitation of previously formed concepts*. This function of symbols may be illustrated by comparing the performance of a highly trained monkey in a situation with and without specific symbols appropriate to the elicitation of two complex organized response patterns (Harlow, unpublished manuscript).

This monkey had been trained to solve the oddity and nonoddity problems to appropriate symbolic cues. The animal was required in the oddity problem to choose the odd one of three stimuli, and in the nonoddity problem, the two like stimuli. When cues appropriate to the training were presented, a black test tray indicating oddity and a green tray indicating nonoddity, and the problems irregularly alternated from trial to trial, the monkey succeeded on 95 per cent or more trials. The same level of performance prevailed when one problem was presented for 25 consecutive trials followed by the other problem for 25 trials.

When this complex problem was presented to the monkey without the symbols to indicate the problem shifts, very different results were obtained. In the first experiment the oddity and nonoddity problems were alternated every 15 trials, but in 50 days of training the monkey gave little evidence of learning to perform effectively. Subsequently, in the course of 50 additional days of training alternating the phases every 25 trials instead of 15, the monkey came to show a considerable ability to respond with the appropriate principle (see Figure 109). But at best the performance without aid of symbols contrasts strikingly with the almost errorless performance in the symbolically supported situation.

It is obvious that the same general principles operate widely in human behavior. Aments, dements, and brain-injured given sorting tests without benefit of symbolic cues typically show one concept but can go no further. But the same subjects often can respond with additional concepts when verbal cues are offered. Likewise, most of Scheerer's subjects failing without symbolic aid in the first sorting test, succeeded when such help was given.

Aided by symbols, monkeys have been trained to perform with great facility on tasks of ostensibly marked complexity. Harlow (1942) trained four monkeys to displace a rewarded sample-object in the left compartment of a food tray and then to select the matching one of two objects in the right compartment. After this principle had been mastered the monkeys were trained to differentiate this from another situation in which the sample object was unrewarded and, as such, served as a symbol for selecting the nonmatching object in the right compartment. The problem was further complicated by

training them to go to the right-position choice-object if the sample-object was rewarded and matched neither of the choice objects, and to go to the left-position choice-object if under this condition the sample-object was unrewarded. If all three objects were identical these last-named relationships between sample and choice-object selections were reversed. In spite of the complexity of the 16 configurational patterns, monkeys did learn to solve almost without

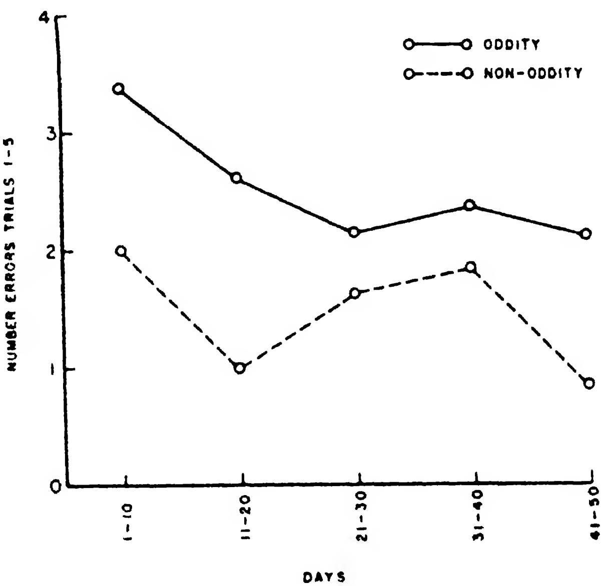

FIG. 109. Performance on oddity and non-oddity problems unaided by external symbolic cues. (From unpublished materials at the University of Wisconsin by permission of the University.)

error these problems presented in irregular order. Moreover, two monkeys with bilateral destruction of the frontal lobes attained a very considerable success.

Weinstein (1945) established in two monkeys a concept of blueness elicited by the symbol of an unpainted circle and a concept of redness elicited by the symbol of an unpainted triangle. The more efficient of Weinstein's monkeys responded to the circle by picking all of the nine blue objects and ignoring all of the nine red objects and responded conversely to the unpainted triangle. Choices were

made irrespective of the size or form of the objects or the brightness or saturation of the color. Objects with small patches or designs of the appropriate color and with far more conspicuous designs of other colors were selected.

It is our opinion that objects such as the circles and triangles and stimulus trays painted various colors can serve as stimuli effective in both the formation and elicitation of organized response patterns. But objects can at best be only a poor substitute for language as a symbolic device No doubt we could train children that triangles stood for dogs and circles stood for cats, but it would be hard indeed to present a triangle simultaneously with every dog and a circle with every cat. And even if our canine and feline friends were to cooperate, the problem would be burdensome as we trained the children to form not two but two thousand concepts Soon we would be carrying sacks of symbols, then bags, and finally truck-loads, if we had not already abandoned the educational process as hopeless. Linguistic symbols are easy to carry about—even in graphic form they demand only pencil and paper Furthermore, words possess another great advantage they can be presented to the learning animal from any position whenever he is oriented to the significant. Beyond this the number of discriminable patterns of sounds approaches the infinite, whereas there is an amazing paucity of readily discriminable colors and forms

All of the data indicate that concepts are much more easily elicited if they have been related to some symbolic cue, and since linguistic cues are the most facile and easily manipulatable symbolic cues, it is obvious that the elimination of language greatly limits the ability of any animal to utilize easily previously formed organized response patterns.

Symbolic cues, however, are no *sine qua non* of effective concept elicitation. There is every reason to believe that other complicated mechanisms may greatly influence the efficiency of the elicitation process. There is evidence that the formation of some generalized principle of attempting systematically to modify and reorganize the stimulus situation may aid in elicitation of previously mastered principles or concepts appropriate to the problem. Illustrations of the efficiency of this process are replete in Wertheimer (1943). In his

parallelogram problems, for example, he shows that solution of new problems is often greatly facilitated by trying out spatial reorientations of the geometric design. Helson and Helson (1946) suggest that the psychological principles valid for the solution of the concrete, easily geometrized type of problem also apply to problems presented in abstract, symbolic terms. As an illustration they explain the solution of an algebraic problem by a process of reorganization of the equations.

From the position of the present author any such manipulation would increase the likelihood that the new situation will elicit already formed organized response patterns which might in turn lead to problem solution. The Gestalt explanation assumes that the restructuring produces a new and better configuration which in itself gives rise to problem solution, but differences in interpretation may possibly reduce to semantic differences.

Problem Solution

Termination of the thinking process may come about as a result of one of three events: (1) the goal is attained, (2) a goal satisfactory to the animal is attained, even though it may not be the logically correct goal, or (3) the problem is abandoned.

In most experiments on subhuman animals the goal is some objective incentive such as food, water, or shock avoidance. Under such conditions the learning-thinking process usually terminates clearly with the goal attained or abandoned. Thus the presence of a clear-cut objective goal enabled Kohler's chimpanzees to continue activity until good errors had been converted into insight.

Many problems faced by both subhuman and human animals are repetitive, with the result that the goal is not an incentive that is either obtained or not obtained but an incentive operating on some proportional reinforcement basis. A particular reaction tendency may lead to perfectly consistent goal attainment. But if the animal fails either to form or to use this reaction tendency there may be one or more possible alternative reaction tendencies that will lead to a frequency of goal attainment that is acceptable to the organism and constitutes problem solution for him.

In comparative psychology there are frequent accounts of animals falling into position habits when faced with more complicated problems involving stimulus object discrimination or delayed reaction. These inadequate reaction tendencies give, of course, 50 per cent reinforcement unless some systematic reward procedure is introduced by the experimenter. The present author observed an interesting example of behavior of this type in monkeys. Normal monkeys consistently learn to solve the oddity-principle problem, but three out of four monkeys with very large cortical lesions tended to select and respond consistently to one of the two objects, a procedure providing 50 per cent reinforcement.

This mechanism is operating in countless human thinking situations. Darwin recognized it in stating the necessity he found to write down all instances of evidence against evolutionary doctrine lest he otherwise ignore them. Much of our learned social behavior is doubtless limited in its perfection by such a mechanism - we behave in ways accepted in a high proportion of instances and abandon our social thinking processes beyond this point. The level of manuscripts submitted to editors also bears evidence of cut-off points in the thinking process, since the authors frequently give up further work when they believe a high percentage of the paragraphs will be acceptable.

In many human learning-thinking situations the goal is the attainment of a "logically correct" answer. Psychological definitions, philosophical theories, mathematical solutions would fall within this category. Frequent failure of expert opinion to agree on the presence or absence of correct thinking indicates the difficulties of assessing problem solution in many instances. There are cases of tens or hundreds of years passing before expert opinion has agreed on the logicality of certain mathematical theorems.

Since many human problems offer no absolute goal, it is only natural that the wishes and emotions of the human being will affect the level of elicited responses accepted as constituting problem solution. This has been demonstrated experimentally by Lefford (1946) who tested 186 college students on 20 pairs of syllogisms matched for form and length, one member of each pair being emotionally toned, the other nonemotionally toned. Analysis of the results showed that the subjects solved the neutrally toned syllogisms more adequately

than those syllogisms emotionally toned. The validity judgments of the nonemotionally toned syllogisms fell into a normal distribution curve whereas the emotionally toned syllogisms gave a J-shaped distribution with a pronounced piling up of scores at the low end of the distribution.

Clinical literature is, of course, replete with accounts of emotional and motivational states leading to the acceptance of inadequate hypotheses. Crawshay-Williams (1947) has recently written a book in which the motivation for irrationality is traced to self-preservation, reproduction, and gregariousness. One of the basic techniques described for avoiding frustrations is that of adopting comfortable concepts.

Comfortable, emotionally-satisfying, autistic thinking may involve elicitation of complex concepts, and it may involve rearranging them into new and varied settings. Regardless of its intellectual sophistication, it is, however, characterized by acceptance of a problem as solved when the solution is not in keeping with the accepted solution level of the social group or with reality.

This willingness to accept a problem as solved at an inadequate level no doubt pervades much of our other thinking. Thus Alpern (1947) found that the ability of high school students to test scientific hypotheses depended in part upon a "habit of delayed response" and was not highly correlated with intelligence or number of previous terms of high school science training. Such data suggest again that complicated habits and critical attitudes toward problem solution level may influence efficiency of thinking

The thesis which the author has proposed assumes that appropriate cues elicit in the organism habits or organized response patterns until he accepts the problem as solved or abandons the process It is believed that the habit of delaying acceptance of problem solutions is one of the important factors determining efficiency of thinking. In the human being it should be noted that these elicitation and selection processes may continue for days, weeks, or years. This affords a vast opportunity for previously learned habits and concepts to transfer, to generalize, and to be associated in new ways without violating the fundamental law that temporal contiguity is one of the

essential conditions for the formation of all new associations. It also affords opportunities for new learnings that may aid in solution.

It is possible that the Gestalt learning theorists imply nothing more than such processes when they state that productive thinking or reasoning involves reorganizing or restructuring past experience. It would appear, nevertheless, that their insistence upon the duality of learning and thinking as opposed to the unitary principle espoused in this chapter would preclude this possible basis for concurrence.

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CHAPTER 11

SUREMENT IN PSYCHOLOGY

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The process of measurement lies at the very heart of all empirical science. A brief glance at the history of science reveals the fact that refinement of principles and techniques of measurement has been a most important factor in its development. Today, those scientific disciplines that have achieved the highest state of development have been most successful in overcoming the obstacles to precise measurement. This suggests the possibility that the future of psychology as a science will depend very much upon the success with which its own peculiar problems of measurement are solved.

WHAT IS MEASUREMENT?

Measurement is so common in everyday life that almost everyone has had some experience with it. The housewife buys her vegetables by the pound, children proudly note their increasing stature by using the yardstick or tape measure, and family providers continually meet with the results of measurement in terms of kilowatt hours, cubic feet of natural gas, or gallons of fuel oil and gasoline consumed. Even the more technical measurements of physics, such as those of resistance, inductance, capacitance, and velocity, are familiar to many individuals from the practical world of affairs.

Definition of Measurement.—A great deal of controversy has been centered about the definition of the word 'measurement.' [1]

[1] Single quotation marks will be used to indicate when the discussion concerns a word itself rather than that which the word denotes.

Many definitions have been given, some better than others, but none of them false, for the manner in which a word is used remains a matter for decision by the user So long as he indicates clearly what he means by a word when it is used, the investigator is entirely within his rights Some physicists have objected strenuously to certain uses of the term 'measurement' by psychologists and other social scientists, claiming that the processes to which the word is being applied are not measurements at all Argument of this sort is futile, for the only kind of conclusion possible is one by common consent

Actually, we shall not attempt a precise definition because it is useful to apply the term 'measurement' to a wide variety of processes which have certain characteristics in common Since there is good reason to believe that much remains to be done in developing methods of measurement in psychology, there is little to be gained by stating precisely the range of procedures to which the term is applied, because prompt revision of such a definition would certainly be necessitated by the rapid improvement that is being made in the field Whatever the final definition of 'measurement' may be, most of the instances in which the term is now applied have the common characteristic that *numbers are used to represent the results of certain operations that have been performed* The operations of the scientist, more specifically, are in the form of observations of some kind Numbers are used as descriptive labels for things observed

Two Major Types of Measurement.--The kinds of situations in which numbers are employed to describe the results of operations can be divided into two large categories The first of these groups is composed of all those situations in which discrete objects or events are considered, for which the fundamental operation is that of counting The second group deals with instances in which the properties or events to which numbers are assigned vary in a continuous fashion In the case of the latter category, there is no single operation by which numbers are assigned to properties of objects or events Indeed, the field of possible operations in the realm of continuous properties seems to have no end

The data that fall into these two major categories are called "enumerative" and "metric," respectively Frequently, only the

assignment of numbers to metric properties is considered to belong to measurement proper However, there seems to be little reason to exclude the assignment of numbers by the operation of counting, because, in both cases, the end result is a representation of operational procedures by the use of numbers Actually, the operation of counting discrete events is employed frequently in psychology for the assignment of numbers to continuous properties, as in the case of certain mental tests, where the total score is obtained by counting the number of items passed Many other instances are to be found among the psychological scaling methods

How Measurement Came into Psychology

Two main lines of development in the history of psychology have contributed to the practice of measurement as it exists in this field today. The psychophysical tradition, originating with Herbart, Weber, and Fechner, was developed largely in Germany, while the study of individual differences, stemming from the work of Gauss, Quetelet, Galton, Cattell, Spearman, Thurstone, and others, owes much to British genius. Psychophysics, of course, was the forerunner of modern experimental psychology, in which many problems of measurement have arisen.

Psychophysics.—As early as 1825, Herbart attempted to employ mathematical equations to describe the interaction of two ideas of unequal strength in consciousness Although his actual formulations are of no practical significance today, it was he who first suggested the possibility of using mathematical methods for the description of mental phenomena, a suggestion which later played an important role in the founding of psychophysics Herbart also introduced the concept of an absolute threshold.[2] A very short time later, Weber reported the famous relationship which states that the just noticeable increment in a stimulus is proportional to the stimulus. This law can be expressed symbolically as follows:

$$\frac{\Delta R}{R} = K$$

[2] Herbart spoke of the threshold of consciousness as a degree of strength which ideas must have to remain conscious.

In this equation, R represents the stimulus value, ΔR is the just noticeable increment in the stimulus, and K is a constant.

Little significance was attached to Weber's law, which was not even stated in mathematical terms originally, until Fechner used it in developing his famous psychophysical relationship:

$$S = C \log R$$

where

S = the intensity of the sensation.
R = the strength of the stimulus given in terms of the absolute threshold stimulus as the unit.
C = a constant multiplier depending upon the type of stimulus and the individual observer.

This law, sometimes called the Weber-Fechner law, states simply that the strength of the sensation is proportional to the logarithm of the stimulus. Much debate has raged, pro and con, concerning the validity of this law, but whether it stands or falls, it will remain a great milestone in the history of psychological measurement, for the Weber and Fechner laws represent the first successful attempts to describe psychological relationships in mathematical terms.

In the attempt to establish his law, Fechner developed the foundations for all the important psychophysical methods. Directly attributable to him are the *method of average error,* the *method of minimal changes* (*method of limits*), and the *constant methods,* all of which permit numerical descriptions of sensory data. Following Fechner, important contributions to the field of psychophysics were made by such outstanding men as Wundt, G. E. Müller, Urban, Culler, and Thurstone.

The significance of this movement, stimulated largely by Fechner's work, cannot be overestimated in the development of mental measurement, for herein lies the first successful attempt to employ numerical expressions to describe the results of operations with psychological judgments, previously supposed to lie beyond the reach of such treatment. Besides being the first successful instance of quantification in the realm of mental phenomena, psychophysics stimulated the development of experimental psychology, which has seen

the extension of quantitative methods to an ever increasing number of psychological variables.

Individual Differences.—The other main source of quantitative methods in psychology, the study of individual differences, can perhaps be traced as far back as Bernoulli (1654-1705), who laid the foundations of probability theory, and DeMoivre, who originated the normal distribution curve, about 1733. Early in the nineteenth century, Laplace wrote his masterpiece on probability, containing proof of the method of least squares, so important in statistics, and Gauss demonstrated the practical value of the normal probability curve, showing how it applied to the distribution of measurements and to errors made in scientific observations. It was he who devised the fundamental methods of computing means, probable errors, and other well-known statistics.

Quetelet, royal astronomer to the King of Belgium, was the first to apply the normal curve and elementary statistical methods to biological and social data, such as births, deaths, marriages, diseases, crimes, and anthropometric measurements on unselected populations. This development, which showed how mathematical equations could be employed in the description of social phenomena, occurred a few years before the middle of the nineteenth century.

The last decades of that century saw the application of these results to truly psychological phenomena in the pioneer work of Sir Francis Galton, who originated the mental test, the rating-scale method, certain statistical tools such as standard scores, the median, and the idea of correlation. The tests used by Galton to measure individual differences in physical and mental traits were generally of the simplest sensory or motor type, most of them being readily scored in physical units. For sensory tests, the stimuli that could be discriminated were calibrated in terms of units of the stimulus, while for motor tests, units of work completed or time required to complete a certain task served as criteria for evaluating performance.

Considerable impetus was given to the study of individual differences here in the United States by James McKeen Cattell, who was perhaps America's greatest pioneer in the mental-testing field. He made many important contributions to the literature on reaction

time, attention, association, and the perception of small differences. Not only for his own work, but for his influence on many students, Cattell deserves much of the credit for the interest in quantitative description of individual differences which exists today.

Important as the contributions of Galton and Cattell were in laying the foundation for a quantitative psychology, much of the credit for present-day mental-measurement methods must be given to Ebbinghaus and Binet. It was Ebbinghaus, in his classical studies of memory in the early 1880's, who opened up new avenues of measurement, and therefore of experimental possibilities. It was Binet who conceived the important innovation of attempting to measure individual differences in the more complex functions, such as reasoning, comprehension, memory, and suggestibility. Prior to Binet's work, the variables that were measured had little significance, in terms of predicting the adjustment of human beings in various behavior situations.

From the concepts originated by Binet, mental measurement rapidly expanded in scope until tests were developed not only of intelligence, but also of interests, attitudes, temperament traits, and of many special aptitudes. In recent years, the tendency has been to devise tests that measure separate and distinct abilities and other traits,[3] rather than constellations of such variables, as in the measurement of intelligence. But the emphasis has remained upon the measurement of important adjustment variables rather than upon sensory and motor variables.

This shifting of emphasis has been guided largely by the practical problems of predicting human behavior. With the early sensory tests, it was not possible to predict whether a particular individual would be a success in school, on the job, or in marriage. By concentrating on the more pertinent traits, psychologists have been able to develop tests that make possible predictions of human behavior in these and other complex adjustments.

Psychological Scaling.—A third source of measurement methods in psychology has been in the interest of evaluating responses to

[3] In this discussion, 'trait' is defined so as to include ability. In its broadest sense, a trait is any observable variable among individual differences.

stimuli. In psychophysical studies, there has always been interest in relating response values (just noticeable increments, average errors, and the like) to absolute stimulus values. These response values, however, have often been given in terms of some physical scale. The "just noticeable increment" is described as a distance on the scale of physical energy and the "average error" is a standard deviation or some other measure of dispersion, also on a physical scale. Such measures are sufficient for testing the applicability of Weber's law or its substitutes, as well as for other experimental purposes.

Note, however, that Fechner's law contains an S value as well as an R (stimulus) value. The S value is on a *purely psychological scale* which calls for psychological units rather than physical units. The loudness of an experienced sound is on an entirely different continuum from the amount of physical energy. The pitch of a tone is on an entirely different continuum from that of wave frequency. The lightness or brightness of a light is on a psychological continuum, whereas variation in light-wave energy is on a physical continuum. Psychological scaling, as distinguished from psychophysics, in general, has as its goal the evaluation of a psychological response on some scale of psychological properties and with psychological units.

The scaling methods originated with Fechner in his study of aesthetics from an experimental approach. He wanted to know how much pleasure or how much aesthetic value, on the average, people attached to different objects—rectangles of different proportions, divided lines, colors, paintings, and the like. The degree of like or dislike for an object is conceived as lying on a psychological continuum which may be called an affective scale or an aesthetic scale. The position of any object on such a scale may be called its affective value or its aesthetic value.

Scaling methods are not confined to affective and aesthetic evaluations. Responses can be evaluated on almost any definable continuum; the intensity of sensations of any kind, the length or size of objects, the complexity of an object, the quality of handwriting specimens, the quality of English compositions, and so on. The psychological values to be associated with certain objects are usually derived indirectly from human judgments of some kind. We cannot

go into procedures here, but the more common ones will receive mention later in this chapter. Among the types of judgments which serve as sources of information for scaling are the following: Object G is better balanced than object E; object Q is twice as complicated as object M; tone V is half as loud as tone W; color S is as much more saturated than P as color L is than J; if I could have reward R, I would be willing to take punishment T. From such reactions, by applying certain sets of operations it is possible to attach meaningful numbers to the various objects. The evaluation of objects is thus not restricted to simpler sensory or perceptual properties, as was true in the traditional psychophysics, but can be extended to the determination of moral, social, and economic values as well as the affective and aesthetic values with which Fechner started.

In Summary.—This quick review of the history of psychological measurement, and of its more current ramifications, has been given to set the stage for a much less superficial discussion, including a consideration of the logic underlying the subject. Most psychological investigators have been so much more concerned with their immediate experimental or practical problems that they have taken little thought to the logical foundations of what they are doing. Their neglect in this respect has not by any means passed unnoticed. There have been many critics who have pointed out that, by comparison to measurement in the physical sciences, psychological measurement is not really measurement at all. A few psychologists who have been more sensitive to these criticisms than most, have taken time to give the matter some thought. The following pages will give a very brief account of that thinking as well as our own analysis. Its results should help the student of psychology and the general investigator to achieve an improved orientation with respect to measurement.

Measurement and Mathematics

Most students realize that measurement is somehow related to mathematics, but the precise nature of this connection is probably not very clear to them. This state of affairs is unfortunate because the problems of measurement in psychology do not follow the more

regular patterns which have been laid down for the older sciences. Since the problems facing the psychologist are relatively unique, it is impossible for him to find the answers already worked out in mathematical textbooks. Success is likely to be achieved by those who have a fundamental understanding of the principles underlying the application of mathematics to empirical data. With such knowledge, an intelligent attack upon difficult problems in psychological measurement can be carried out.

Pure and Applied Mathematics.—At the beginning, a distinction should be drawn between pure mathematics and applied mathematics. Systems of pure mathematics do not contain any direct reference to the world of experience. They are abstract, or formal, statements in which no direct description of empirical objects is given. In the expression, $a + b = b + a$, for example, it is meaningless to ask what physical things are indicated by the letters. Pure mathematical systems, as such, do not aid us in the task of describing and predicting conditions in our surroundings.

Applied systems of mathematics, on the other hand, are employed to describe actual conditions that exist. A familiar example is the applied Euclidean geometry that most of us learn in high school. The physical system described by Euclidean geometry is the three-dimensional space in which we live. Applied mathematics is useful to us precisely because it does provide a means of symbolic description and manipulation of the objects in the environment.

Although systems of pure mathematics intrinsically do not describe anything known through experience, it is often possible to assign certain interpretations to the abstract symbols and operations of such systems so that the statements can be converted into meaningful propositions about some actual situation. Before this point is discussed further, however, something should be said about just what makes a system a mathematical one.

Postulate Systems.—A mathematical system is a postulate system. A postulate system is composed of a group of logically interrelated statements. Some of these statements constitute the foundation of the system and are called "postulates." All the remaining statements are built up by deduction from the postulates and are

called "theorems." In an applied system of mathematics, the postulates and theorems would contain symbols which refer to existing objects, events, and to manipulations which could conceivably be performed with such phenomena. In a pure mathmatical system, the postulates and theorems would be expressed abstractly in symbols devoid of any empirical reference.

In pure mathematics, the postulates are merely assumed to be true, rather than proved. It would be circular to prove the postulates within any one system because the postulates constitute the premises from which deduction proceeds and as such cannot be used to establish their own validity The theorems in such a system are considered to be true if they follow logically from the postulates That is, the theorems constitute merely the deductive elaboration of what is implied by the postulates assumed It follows, therefore, that the kind of truth obtained within a system of pure mathematics is a logical truth rather than an empirical truth. There is no appeal to experiment or observation, nor can there be It would be impossible to establish the validity of a theorem in a system of pure mathematics by experiment, for empirical results have no part in the construction of such systems

An applied system of mathematics is usually constructed in the form of a postulate system, too. Certain statements are taken as postulates and others are derived by logical and mathematical reasoning from them. Since applied mathematics is supposed to be useful in describing actual conditions in the world, however, the logical truth found in mathematical systems of the pure type is not sufficient That is, it is not enough that the theorems be derivable mathematically from certain assumed postulates, but all the statements, both postulates and theorems, must constitute propositions which accurately describe empirical situations.

The postulates of such applied systems are usually carefully chosen because they represent statements that can be proved to hold empirically for a given context of experience or that appear certainly to be true, even though no possibility of verification exists. When such a system of postulates has been chosen, their implications can be elaborated deductively to obtain theorems which will presumably still be true in an empirical sense That is, if we start with true

statements, and find what follows logically from them, the resulting propositions should also be true in more than the mere logical sense.

The essence of a mathematical system, then, is not to be found in the complicated symbolism that baffles so many students. It is rather in the form which description takes. A postulate system composed of verbal statements can be a mathematical system just as much as the differential and integral calculus. Perhaps the ultimate aim in any science is to express its known facts in the form of a mathematical system, sometimes called a "hypothetico-deductive system."

Applying Mathematical Systems to Experience.- One might wonder from the previous discussion what the value of pure mathematical systems is, since they do not appear to tell us anything about the world. The fact is that many pure mathematical systems can be "interpreted" so that they actually become applied systems describing important segments of experience. The abstract relations that appear in a pure mathematical system may have the same form as corresponding relations among physical phenomena. If the abstract symbols and operations of the pure mathematical system are "interpreted," i.e., rules are stated by which they are referred to corresponding physical phenomena and their relations, the pure mathematical system is converted into an applied, or physical, system. Instead of expressing abstract relations among symbols without empirical content, the postulates and theorems of the system now express statements about observable phenomena and as such can be put to experimental test.

In interpreting some mathematical system, the ideal procedure would be to convert all the pure postulates into their empirical counterparts and prove them by experiment. The theorems, then, should follow, just as they do in the abstract system. It would always be necessary to check such theorems with the facts, however, to be sure that the total system was actually consistent with the facts. Errors in verifying the postulates and unwanted hidden assumptions are easily introduced.

The Number System.—The preceding discussion has a bearing upon measurement because the number system itself is a pure mathe-

matical system which we wish to apply for the description of empirical phenomena, including human behavior. Thus, if numbers can be assigned to represent quantities, psychological or otherwise, and the operations with numbers can be related to experimental procedures with quantities, the statements of arithmetic may be employed for the description of quantities and their relationships to each other. Furthermore, manipulations of such quantities may be carried out symbolically rather than physically.

Before the number system may be employed to describe quantities of some particular variable, however, it is necessary to demonstrate that the variable possesses the kind of intrinsic structure which the number system itself has. That is, there must be a similarity of structural relations between the mathematical system of number and the physical system of quantity. The number system is to be interpreted in terms of quantities and operations with quantities. Before discussing this point further, it is necessary to know more about the number system itself.

Number Systems.—Instead of speaking about *the* number system, we should actually discuss number *systems*, for there are several kinds. Perhaps the simplest type of number system is that of the signless integers, 1, 2, 3, and so on. This system would be inadequate for many purposes of numerical description because subtraction and division often yield results which cannot be expressed as signless integers. Thus, if an attempt is made to subtract the number 5 from the number 3, the result cannot be expressed in this system. Also, the number 5 cannot be divided by the number 2 to obtain an integer.

To remedy these difficulties with the system of signless integers, extensions of the number system were introduced. First, the negative and positive numbers, with zero between them, were formulated. In this system, subtraction within the system is always possible, but the difficulty with division is still not eliminated. To make division possible in all cases, the fractions were added. The joint system composed of the positive and negative whole numbers, with zero, and the fractions, is called the system of "rational numbers." Further extensions are possible, since numbers like the square root of 2

are not included in the rational numbers, i.e., they cannot be expressed as a ratio of two whole numbers. These numbers are called "irrational numbers," and together with the rational numbers constitute the system of "real numbers"

In the actual process of measurement, the system of rational numbers is sufficient. In carrying out calculations upon measurements, irrational numbers may become involved, but for all practical purposes they may be ignored, because rational numbers can be found which will approximate an irrational number to any desired degree of accuracy. Thus, it is feasible to build a theory of measurement on the foundations of the system of rational numbers

Applying the Number System in Measurement.—The system of rational numbers constitutes a pure mathematical system which can be built up from a set of postulates, just as can other pure mathematical systems. In order to employ this system for the description of continuously variable quantities, as we do in measurement, it is necessary to set up rules by which the symbols and operations of the mathematical system of number will refer to empirical quantities and to operations with them Furthermore, once the symbols are given empirical reference, it is necessary to show that crucial propositions of the mathematical system will be valid when converted into statements about quantity. This is in accordance with what has already been said about interpreting pure mathematical systems for the purpose of describing data of experience.

To show that a perfect correspondence exists between a physical system of quantity and the system of rational numbers would require a demonstration that all the postulates and theorems of the number system are truly descriptive. In practice, it is never necessary to go to this extreme, and as a matter of fact, it would be impossible to do so, for there are some postulates underlying the system of rational numbers which could not be verified completely in any empirical context. One other fact which makes it impossible to establish a perfect similarity of structure between an empirical system of quantity and the rational number system is the necessity for making an arbitrary assignment of some one number to some one quantity. That is, in developing a correspondence between a series of numbers

and a continuously variable series of quantities, there is no a priori reason for making any particular quantity correspond to any particular number. Once some one quantity is selected, and the number 1 assigned to it, for example, the numbers to be assigned to the other quantities are automatically fixed, for more complete types of measurement

Although it is not possible to demonstrate a complete correspondence between rational numbers and a system of quantity, structural similarities can be established such that, for all practical purposes, numbers, and operations upon them, can be employed for description In order to demonstrate this similarity of structure, experimental operations must be devised to verify certain key propositions underlying the rational number system

In Summary.— A pure mathematical system is a logical structure with no necessary empirical implications It is constructed by beginning with postulates (propositions or statements) from which other statements are deduced The rational number system is such[1] a mathematical system It is composed of the positive and negative integers and fractions and zero This system, including the numerical operations it provides, is sufficient for what is now known as measurement

A mathematical system may be applied to the description of experienced events when the inner structure of those events corresponds sufficiently with the inner structure of the mathematical system. There are empirical ways of checking whether the correspondence is close enough to justify the application As will become more apparent later, there are many ways in which the appropriateness of the use of numbers in description becomes evident

Applications of Numbers to Data

In this section, more attention will be given to the specific steps that must be taken to apply the number system in measurement It will be pointed out that psychological measurement is not capable of meeting the stringent requirements for a complete application of the system of rational numbers to its data. The nature of this defect will be explained, and some interpretations will be suggested which

still allow considerable application of numerical description in psychology. Much of the criticism aimed at psychological measurement reflects a rather narrow interpretation of the nature of the measurement process. Psychological measurement must be based upon a broader foundation than measurement in the physical sciences.

Requirements for Complete Measurement.—The propositions that will be laid down here constitute a set of minimum requirements that a system of quantity must satisfy before a set of rational numbers can be unreservedly employed to describe it. These statements have been carefully worked out by mathematicians and physicists over a long period of time. The first two propositions define the type of *order* which must exist among magnitudes, or quantities; the next two statements describe the conditions which must be satisfied for *equality* judgments; and the last four are used to outline the characteristics of addition as an operation applied to magnitudes. The eight requirements for complete measurement, then, are as follows:

1. Either x is greater than, less than, or equal to y, but not more than one of these conditions prevails.
2. If x is greater than y, and y is greater than z, then x is greater than z.
3. If $x = y$, then $y = x$.
4. If $x = y$, and $y = z$, then $x = z$.
5. $x + y = y + x$.
6. If $x + y$ is greater than x, then y is not equal to zero
7. If $x = z$, and $y = p$, then $x + y = z + p$.
8. $(x + y) + z = x + (y + z)$.

In order to show that any quantity can be measured in a complete sense, i.e., a system of rational numbers can be fully employed for the description of relations among quantities and relations to other variables, it is necessary to provide experimental interpretations by which these eight requirements can be verified. The relations, "greater," and "equal," must be defined in terms of operations with quantities. The same requirement applies to an interpretation of the arithmetical operation of addition.

Suppose there is a common characteristic by which a class of quantities can be grouped together, such as with the class of objects having length. Length, then, can be considered a magnitude or quantity to be measured. The unknown values in the eight statements, x, y, z, and so on, will represent magnitudes of length. How can it be shown that length can be measured in a complete sense?

Fulfilling the Requirements for Measurement.—To take a simple case, by way of illustration, consider the length of lines. The relation, "greater," is defined as existing between lines x and y, when line x extends beyond line y when x and y are laid side by side, both starting from equivalent points. The relation, "less," is merely the inverse of the relation "greater," so if x is greater than y, then y is less than x. The relation "equal" will obtain between two lines if the two are congruent, i.e., if laid side by side, they begin and end together with no overlap by one line. The arithmetical operation of addition will be interpreted in the physical sense as the process of laying lines end to end.

We are now in a position to see whether the eight requirements are satisfied by the magnitude "length of lines." The first condition is satisfied because when two lines are placed side by side, we can always tell that one is greater than, less than, or equal to the second, within the limits of observational error, and that only one of these relations holds between the first and the second line. The second requirement is verified by selecting two lines, x and y, of which x is the greater, and another line, z, such that y is greater than z. Then, if x is actually greater than z, by the same criterion, the condition for proposition *2* is satisfied.

The third requirement is obviously satisfied by our method of obtaining equality of lines. There is no possibility of judging one equal to another without the reverse situation being true, also. Furthermore, lines equal to the same line will prove to be equal to each other, as required by the fourth statement. To verify the fifth condition of measurement, place two lines end to end and find another line that is equal to the sum of the first two. Now, add the first two lines in the reverse order and see whether their sum is still equal to the other line. If so, the fifth requirement is satisfied. By similar

procedures, the other requirements can be shown to hold for this variable. In these examples, it has been assumed that if the conditions hold for a number of values sufficient to make the test, they will hold for all other quantities which could have been chosen. In practice, a sufficient number of tests would be made to reach a satisfactory degree of certainty such that further testing would be unnecessary.

Assigning Numbers to Quantities.—Once the requirements for measurement have been satisfied in this way, the actual process of measurement may proceed, i.e., numbers may be assigned to the various quantities, in this case, lengths of lines. First, it was necessary to be sure that the relations "greater" and "equal" as well as the operation of addition, have certain characteristics which apply to the number system, itself. That is, a similarity of structure was being demonstrated between the magnitude, lengths of lines, and the system of rational numbers Once this has been done, the physical operation of addition, in this case laying lines end to end, can be employed for the purpose of assigning numbers to represent various lengths of lines. Furthermore, the numbers thus assigned can be manipulated in accordance with the rules for arithmetic with the full expectation that such manipulation will faithfully represent actual physical manipulations which can be carried out with the lines themselves, albeit not so easily.

It might seem strange that all the arithmetical operations could be performed with such numbers because the eight requirements for measurement stated include only properties of addition The fact is that the other arithmetical operations can be defined in terms of addition, and this is what we do in measurement Suppose we wish to know the result of an operation with lengths of lines corresponding to the arithmetical operation indicated by the expression, $x - y$. Let z stand for the result, whatever it is. Then, z is the value which must be added to y to obtain x. Thus, subtraction is defined with reference to the operation of addition. The multiplication of two lengths of lines, $x \times y$, will be defined as the addition of x lines of length y, or vice versa. Division is then defined as the inverse of

multiplication, and since multiplication is defined in terms of addition, so is division.

The process of assigning numbers to lengths of lines, which is measurement, will proceed as follows. First, one particular line will be selected to which the number 1 will be assigned arbitrarily. Such a selection is always necessary when dealing with continuously variable quantities. Then, another line is found which equals the first. These two are added together and another line found equal to the sum of the first two. This line is assigned the number 2. The standard line of length 1 is now added to the line of length 2, and another line found which equals the sum. This line is assigned the number 3. By this process of successively adding the standard line of length 1 to each larger length, a standard series, or scale, can be built up for lines of integral length.

To find the length of any line which is not one of the original series, it is compared with various values of the scale until one is found to which it is equal. Then the same number is also given to this line as was originally given to the line of the established scale to which it is equal. Some lines for which numbers are desired, however, will fall between the integral lengths established. To remedy this difficulty, the scale must be "filled-in," so to speak. To do this, a second standard line is chosen such that when it is added to itself nine times the result will be a line of length equal to the original standard of length 1. This second standard is then assigned the number .1. When added to itself successively, the resulting lengths will be .2, .3, and so on up to 1.0, and beyond. By extension of this procedure the scale can be built up with any desired distance between adjacent values.

This procedure of assigning numbers to magnitudes guarantees that the quantities represented by different numbers will bear approximately the same relationships to each other in terms of quantity as do the numbers themselves in a strict mathematical sense. That is, if two magnitudes or quantities are given the numbers 10 and 5, respectively, the magnitude to which 10 is assigned will be twice as great as the one to which 5 is assigned. Furthermore, equal numerical differences will represent equal differences of quantity. In general, the ordinary relations that exist between numbers them-

selves will be duplicated among the quantities which those numbers are employed to represent.

The kind of measurement described so far in this chapter is a complete or fundamental type of measurement which is possible in the case of only a few variables. Some examples are: length, weight, period of time, and electrical resistance. In psychology, we rarely, if ever, have measurement that conforms to this complete set of requirements Even in physics, such measurement is not always possible. For example, if the operation of pouring bodies of liquid together is defined as addition for the variable of density, it can be seen that the sixth requirement of measurement is not satisfied. This is true because the density of two bodies of distilled water added together is the same as that for either separately. For density, then, measurement is not possible by the fundamental process described.

The Operational Nature of Measurement. -It has already been shown how fundamental measurement is based upon an interpretation of key relations among numbers in terms of actual physical operations relating magnitudes These particular operations are important ones because they make possible the construction of a scale of measurement in which the numbers assigned embody relations actually existing among magnitudes so faithfully that the ordinary arithmetic operations upon numbers represent manipulations which could be carried out with the magnitudes themselves.

The question at hand is whether it is possible to devise sets of operations which will give measurement that legitimately goes beyond the ordinal level (in which numbers indicate merely rank positions) without reference to an experimental operation of addition. We know that equal units and equal ratios of magnitudes can be set up if a process of addition can be defined operationally for a magnitude, but can these properties be injected into numbers assigned in measurement by means of operations *not* involving addition? Many physicists would say no, but the position to be advanced here is that it can be done. Indeed, it must be possible if psychological measurement is to justify the many procedures in use today which depend upon such properties.

The fact that number meanings in fundamental measurement itself are an expression of the set of operations which have been performed provides the key to the solution of this problem. It points out that *the meanings given numbers assigned in any type of measurement are merely an expression of the operations performed.* Thus, there are as many meanings that can be invested in assigned numbers as there are sets of operations by which the assignment is made. Those who maintain that a process of addition is necessary in order to carry measurement beyond the rank-order level for any magnitude have established a dichotomy, so to speak, among measurement methods. Either they are instances of ordinal measurement with no significance attached to intervals and ratios among measurements, or they have experimental processes of addition from which all the familiar relations among numbers are shown to be applicable to the quantities which those numbers represent.

The attempt to create a dichotomy among measurable phenomena does not fit in with the operational interpretation of measurement outlined here. The numbers assigned to represent phenomena of experience can vary in their meanings, theoretically at least, all the way from nothing in common with the mathematical meanings of numbers to a perfect correspondence with those mathematical meanings. An example close to the first extreme is that of using numbers to identify elements of some group, such as prisoners, or football players. The numbers do not convey any information of a quantitative nature concerning the objects themselves; they are mere tags. While there is no example which meets the other extreme, fundamental measurement probably comes fairly close. Between these extremes, number meanings will vary considerably, depending upon the nature of the operations designed for the method of measurement.

How can the numerical properties of equal units and equal ratios be developed when these depend upon addition? The physicists objecting to psychological measurement processes have assumed that, because these properties can be derived on the basis of an experimental operation of addition, this is the only way of deriving them. Seductive as this assumption may be, it is not valid, for other courses of action are available. Instead of establishing experimentally the operation from which equal units can be derived, the property of

equal units can be attacked *directly*, just as the equality relation is attacked directly in fundamental measurement. It must be realized, however, that such equal units are not of the same kind as those obtained in fundamental measurement. If equal units of the fundamental kind were established, an operation of addition would be implied. What psychological measurement develops is a set of operations by which a definite operational meaning, albeit a new one, is attached to the concept of equal units. This is the fundamental principle upon which psychological measurement must proceed if it is to develop measurement methods that embody more meanings in numbers than are obtained with ordinal measurement.

In fact, this principle is actually extended beyond the limits of meanings to be found in numbers from the strict mathematical point of view. That is, by certain operations of measurement used in psychology, meanings may be given the numbers that have no counterpart in common measurement scales An example of this concept is the centile scale. Within a well-defined sample, a distribution of measurements may be obtained, which, strictly speaking, represent only a rank-order, or ordinal, scale. Each raw score may be converted into a centile equivalent by determining the proportion of individuals in the sample falling below that particular raw score A raw score of 26, for example, would be converted into a centile value of 30 providing 30 per cent of the individuals in the sample have raw scores below 26. The significance attached to intervals on such centile scales is quite definite, but entirely foreign to anything applying to intervals among numbers as such The difference between centile values of 50 and 70 indicates that 20 per cent of the individuals in the sample have scores in that range. "Equal" intervals on such a scale are indicative of equal proportions of individuals falling in those ranges rather than giving equal units of magnitude for the variable measured. Other operations will be discussed later by which properties properly associated with numbers can be brought into psychological-measurement scales.

Measurement without Addition Limited in Scope.—Lest the impression be created that measurement without an experimental process of addition is just as complete as fundamental measurement,

it should be emphasized that such is not the case. Without addition, the application of arithmetic for the expression of relationships among quantities is definitely limited. It is doubtful whether the multiplication, addition, division, and subtraction of measurements will yield accurate determinations with strict quantity interpretations unless a process of addition can be defined. At least, it would be difficult to demonstrate that such operations are legitimate. Thus, where less complete forms of measurement are employed, involving direct verification of certain number properties for quantities, extreme caution must be exercised to avoid interpretations of measurements that are not justified by the operations performed. It would be absurd, for example, to say that an individual with a centile score of 60 is twice as gifted in some measured ability as another individual with a centile score of 30, for there is nothing in the operations to justify such an interpretation.

Statistics and Measurement.—Most of the measurement situations in psychology involve the application of statistical methods to data for the main purpose at hand. We may want to know the differential effect of certain experimental conditions upon the scores of comparable groups of subjects, or we may want to know the correlation between a certain test and a practical performance criterion. These and other questions can be answered only through the application of statistics to the data of measurement.

The calculation of means, standard deviations, and product-moment correlation coefficients, which underlie so many of the statistical procedures applied in psychological research, presume that a unit of measurement exists which is equal along the scale. This fact suggests that it is extremely important that measurement procedures be employed that satisfy this requirement.

The critics of psychological measurement would say that this is impossible without satisfying the requirements for addition. The discussion earlier in this section should go a long way toward discounting such objections, but there is a further point to be considered before such charges can be dismissed. In calculating means, it is necessary to add up individual measurements and divide them by the number of cases. Does this not presume an operation for

addition? This cogent objection has not received much attention in previous discussions of measurement, but it should be resolved if psychological measurement procedures are to be placed upon a firm foundation.

There seems to be little doubt that a quantity interpretation of the sum of a group of measurements would demand an experimental process of addition. However, this is not what appears to be involved in the calculation of the mean. No actual empirical interpretation of the sum of measures is made, nor is it necessary, for the mean is one representative number that summarizes a group of measurements. Only the addition of numbers is involved in obtaining this representative value, not of quantities, and as such the value should not be misleading as long as a legitimate interpretation is available for the relative sizes of intervals between measurements upon which the mean is based.

For this reason, the failure of the less complete forms of measurement in psychology to yield legitimate interpretations of the arithmetical operations does not seem to be a fatal difficulty. If operations can be devised for developing scales with equal units, the usual statistical procedures should be applicable with a sufficient degree of accuracy to warrant their use.

It is also advantageous to develop scales of measurement in which statements that one psychological magnitude is twice as great as another are based upon actual operations. Such scales have absolute zero points as well as equal units, as a rule, and as such come close to the type of measurement that is found in fundamental methods of measurement. For much of the work in psychology, however, such scales are not as vital for the purposes of experimentation as are scales with equal units. Only a very few additional statistical operations are made possible by the extension of number meanings to include ratio determinations, while a great many of the statistical procedures depend on equal units.

In Summary.—The requirements for complete measurement compel us to be able to say several things concerning the quantities to which numbers are assigned. For example, if one quantity is assigned a number higher than that assigned to another quantity, the

first of these quantities must be greater than the second. Two quantities may be assigned the same numbers only if they are known to be equal. Furthermore, it should be possible to show experimentally, as by such operations as placing lines end to end or side by side, that these conditions are satisfied. A crucial requirement for complete measurement is that of addition, since all the number operations depend upon it. It must be possible to demonstrate addition by actual manipulation of quantities in order to justify fully the unrestricted use of numbers.

Probably no psychological measurements satisfy fully these requirements for complete measurement, nor do many physical measurements. This does not mean that there is no ground between measurement and no measurement. There are all degrees of fulfillment of the requirements for measurement. The more fully the experimental operations duplicate the properties of numbers, the more meaning and utility measurements possess. Furthermore, it is possible to show that certain properties, such as equality of units and of ratios, are approximated by means of other experimental operations than that of addition. Only on such a logical basis are many of the statistical operations commonly used in psychology to be justified.

In the remaining sections, a number of representative types of measurement in psychology will be discussed in the light of the preceding conclusions, for the purpose of pointing out what meanings may be legitimately attributed to the numbers assigned.

Types of Measurement Scales

While it has been pointed out that there are as many types of measurement scales as there are operations for measurement, some of these methods are sufficiently alike to warrant their being grouped together as one kind. Stevens (1946) has recently suggested several classifications for measurement scales which he calls "nominal," "ordinal," "interval," and "ratio," respectively. The first of these refers to the instance where numerals are assigned merely as identification tags, hence it does not really represent a type of measurement. The other three kinds of scales will be discussed.

Ordinal Scales.—The ordinal scale represents the type of measurement in which only the first four requirements of measurement mentioned earlier have been satisfied. The relations "greater" and "less" are shown to have certain properties, and an equality judgment of proper specifications is also established Examples of this type of measurement are very common in psychology. One instance is the type of scale obtained by arranging objects in order of increasing lightness. A number of paper slips with equal hue and chroma but varying in lightness may be presented to the subject with the instructions to arrange them in order of increasing lightness. Numbers may be assigned to the various samples as follows: the sample of lowest lightness will be assigned the number 1, the next lighter the number 2, and so on up to the lightest sample. Lightness values may be given to other samples of similar characteristics by finding a member of the ordered group to which each is equal in lightness Even though the numbers thus assigned are equally spaced, this does not mean that each sample is as much lighter than the one before it as that one is lighter than the preceding one. Nothing in the operations performed yield any determination of this kind

For measurements of this type, only a few statistical operations are justified, namely, those which do not presume a unit of measurement. According to Stevens (1946), the permissible statistics with this type of scale are the number of cases (frequency), the mode, contingency correlation, medians, and centiles Under some conditions, rank-order coefficients of correlation may be legitimately applied. Frequencies of ranks would differ from 1 only when two or more objects are given the same rank. When the same object has been assigned rank numbers by several observers, or more than once by the same observer, a mode (most frequent rank) can be ascertained. A median of the ranks assigned to each object can also be determined, but this median value is also a rank number; usually nothing more.

Interval Scales.—In addition to satisfying the requirements for ordinal scales, the methods of measurement resulting in interval scales provide some procedure for establishing an equal unit along the scale of measurement. Many different procedures have been de-

vised for this purpose in psychological measurement, some of which will be described in the next two sections Fundamentally, the method of attack has proceeded along two lines First, a direct experimental attack has been made to develop units that are judged equal by an observer, as in the *method of equal appearing intervals*,[4] and secondly, an attempt has been made to develop equal units by making certain statistical assumptions, as in the case of standard scores for tests of ability and achievement

To the extent that these and related methods are successful in actually developing scales that have equal units with respect to psychological magnitudes, it is legitimate to calculate means, standard deviations, and product-moment correlations, in addition to those statistics permissible with ordinal scales These particular statistics are basic to many of the important procedures for prediction and for drawing experimental conclusions from the use of sampling statistics (standard errors, critical ratios and the like)

With such scales in addition to knowing that large numbers denote greater psychological magnitudes, relative differences among assigned numbers convey information concerning corresponding differences in magnitude separating scale values It is this fact that makes it legitimate to compute the additional statistics mentioned, for these procedures are based upon comparison of the relative differences separating scale values as well as their *order* of magnitude

With interval scales, it is also possible to apply equations relating one variable to another In other words, it is possible to state quantitative laws in mathematical terms This extends considerably the possibilities of making accurate predictions This application of mathematical operations cannot, in the ordinary sense, be made with ordinal values On the other hand, not all types of equations, such as logarithmic functions, can be used with interval values alone The use of the latter requires also an absolute and meaningful zero point This we find in the type of scale to be described next

Ratio Scales.- Procedures of measurement that allow an interpretation of the absolute ratios of magnitude are classified as ratio

[4] See page 536

scales. On such a scale, a magnitude of 60 is twice as great as one of 30, and three times as great as one of 20. Ratio scales also embody equal units and the properties of ordinal scales as well. Indeed, they are close to the kind of measurement that is obtained by the fundamental process described previously. It is not always clear whether all the arithmetical operations may be employed with complete confidence to manipulate such measurements, however, since no process of addition is empirically defined. Possibly, the error in so doing would be slight, but little evidence for this conclusion may exist.

Examples of methods that are designed for the purpose of creating this type of scale are *fractionation*, the *constant-sum method*, and the *method of doubled stimuli*.[5] The principle additional statistic justified with ratio scales that cannot be calculated legitimately with scales of lower order is the coefficient of variation.[6] As was pointed out above, the achievement of ratio-scale measurements also makes possible the application of a much greater variety of mathematical functions or equations to describe the relationship of one variable to another

In Summary.—There seems to be considerable agreement that most instances of measurement, physical or psychological, can be classified in one of three categories, mentioned in order of completeness: *ordinal, interval,* and *ratio* types. Each one offers more agreements between the properties of objects or events measured and properties of the number system than the one preceding it. Ordinal scales provide only the numerical meanings of "greater than," "equal to," or "less than." Very few of the statistical and general mathematical operations can be legitimately applied to such data. Interval scales enable us to speak of equality of distances between measured phenomena, hence the existence of a constant unit. An interval scale is a minimum requirement for the use of the most common

[5] To be described later; see pp. 537 ff.

[6] The coefficient of variation is given by the formula

$$CV = 100\sigma/M$$

where σ is the standard deviation and M is the mean of the distribution. It gives a measure of the relative variability in a group of measures.

statistics: mean, standard deviation, and product-moment correlation coefficient. It also makes possible the application of many types of mathematical equations for the description of relationships among variables.

A ratio scale enables us to speak meaningfully of the fact that one quantity is so many times as great as another, or a certain percentage larger than another. We cannot do this, for example, in connection with the usual type of test scores. We cannot say, with any real sense, that a score of 50 represents twice the ability of a score of 25 or that a score of 60 represents 20 per cent more than a score of 50. While a ratio scale, which has a meaningful zero point, does not extend very much our use of statistics, it does make possible considerably greater use of general mathematical equations, particularly those involving logarithms. It will be seen that not even the ratio scale necessarily means complete or fundamental measurement, as defined previously, for there may be no experimental operation of addition, which is basic to complete numerical treatment and interpretation.

Psychophysical Measurement

There are many variables of a physical nature that correspond to psychological variables over very wide ranges. For example, as the frequency of a sound wave is increased from about 20 cycles per second up to about 18,000 cycles, a human being with normal hearing will experience variations in pitch, which changes from a low value to a high value. The physical property of wave frequency represents a scale of continuous variation, often called a "physical continuum." The psychological variation is called a "psychological continuum." A psychological variable, like a physical one, is some abstracted property that exhibits changes of a specified kind, which can be illustrated in the form of a straight line. The specified change can thus occur in only one of two directions. The term 'continuum' means merely that the variation occurs by infinitely small steps; there are no gaps, no matter how small.

Magnitudes on a psychological continuum, such as that of pitch of tones, must be measured in terms of psychological *units*, on an

equal-unit (interval) scale, if possible. The corresponding physical scale is graduated in equal units of frequency, but such units do not necessarily follow in perfect agreement with equal units on the scale of pitch. Thus, a difference of 10 cycles per second at a level of 1000 cycles is not psychologically equivalent to a difference of 10 cycles at a level of 100 cycles. Psychologically, the second difference will appear greater than the first. This fact might be suspected from a knowledge of Weber's law, although this law does not hold very well for this particular stimulus-response relationship

The physical measures of the stimulus are employed to make possible an expression of the relationships between the physical and psychological variables But if the psychological values derived are to be scaled in equal units, the unit of the physical scale cannot be employed. Furthermore, a stimulus that is physically twice as large as another may not appear to be so from the psychological standpoint. A new scale is needed, then, by which the psychological values can be expressed with numbers representing the proper relations among them A few methods for obtaining such scales, and their properties, will be discussed.

Equal Sense Distances.—The method of equal sense distances is designed to yield a scale of psychological magnitude in which the adjacent scale values are equally spaced, i.e., the distance between each pair of adjacent scale values is equal psychologically to the distance between any other adjacent pair The fundamental operation for establishing such scales is that of bisecting a given interval by observation. Two standard frequencies may be presented to a group of subjects with the instructions to adjust an oscillator until a third tone is produced which appears to divide the interval between the standard tones into two psychologically equal distances. Several settings for each subject are obtained and an average frequency setting taken for the sample The result shows the frequency value that gives a sound judged to be psychologically at the midpoint between those corresponding to the two standard frequencies. In other words, there are three psychological magnitudes of pitch corresponding to the three frequency values mentioned, and the psychological magnitude corresponding to the experimentally determined

frequency value lies midway between the other two psychological magnitudes.

Numerous procedures can be devised to use this basic operation to build up an entire psychological pitch scale. Stevens and Volkmann (1940), for example, provided subjects with a bank of five keys, the end ones of which produced tones of 200 and 6500 cycles per second, respectively. The other three intermediate keys activated oscillators which could be adjusted to give different tones within this range. The subjects were instructed to adjust the frequencies of the three intermediate keys until the intervals between the successive tones were equal with respect to pitch.

Further instructions to the subjects emphasized the necessity of comparing each interval with each other interval, when the keys were played in both ascending and descending order, before being satisfied with the adjustments. The initial bisection of the large interval frequently needed adjustment after the two resulting intervals were themselves bisected. Each subject made several adjustments for the three keys.

This same procedure was carried out with two other frequency ranges of 40 to 1000 cycles and 3000 to 12,000 cycles. The results from these experiments were used to plot pitch against frequency (on Cartesian coordinates) by making the distances between separate values on the pitch axis equal to each to other. For example, corresponding to a frequency of 200 cycles on the X-axis, an ordinate of a certain size was arbitrarily drawn to represent that magnitude of pitch. Another ordinate was drawn from the 6500-cycle point on the X-axis. The frequencies judged to divide this range into equal sense distances of pitch were marked and the ordinates at these points were drawn to the respective heights which would make the distances between these points exactly equal along the Y-axis. For the other two frequency ranges, similar lines were drawn. It was discovered that these three overlapping curves could be fitted into one continuous function of pitch against frequency through the range of 40 to 12,000 cycles. The function obtained is shown in Figure 11.1. Corresponding to a frequency value of 1000 cycles, the pitch magnitude is arbitrarily represented by a value of 1000 *mels*. The

mel thus becomes the unit of the pitch scale, a small interval that is equal throughout the range of frequencies shown, within the limits of experimental error.

The type of scale obtained by procedures of this sort is an interval scale. If by some additional operations a meaningful zero point (corresponding to no pitch at all), is located, the scale would be similar in this sense to a ratio scale. By the method of equal sense

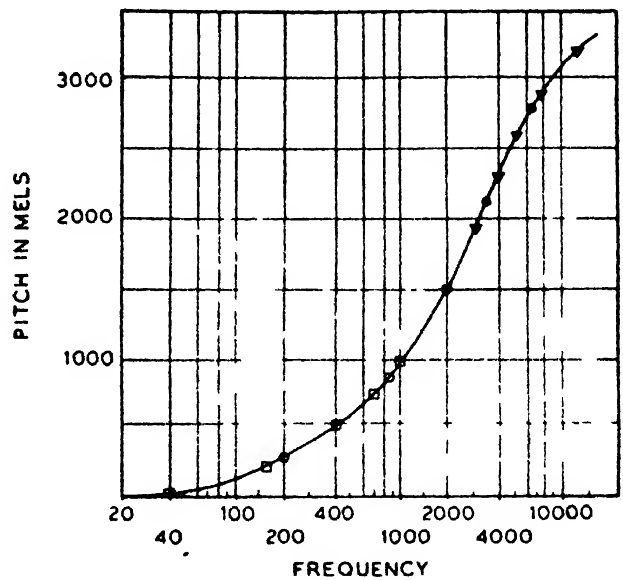

FIG. 11.1. Functional relationship of the psychological variable of pitch of tones measured in mels, to frequency of sound waves, a physical variable (From S. S. Stevens and J. Volkmann, *Am J Psychol*, 1940, 53, p 536, by permission of the authors and The American Journal of Psychology)

distances alone, however, there has been no experimental operation by which equality of ratios has been assured. That property can probably not be assured by the method of equal sense distances alone.

Method of Equal Appearing Intervals.—In the method of equal sense distances, usually only two intervals on the psychological continuum are equated by bisecting a larger interval. By an extension of the principle of comparing intervals, we have a method known as equal appearing intervals.

The experimental procedure is to place at the disposal of the observer a number of objects that he can manipulate—weights he can lift, drawings on small cards, names of individuals on cards, and the like. The observer is asked to sort the objects into piles, often as many as eleven, keeping the steps between adjacent piles psychologically equal. The observations may be repeated by the same observer (if the objects are not of such a nature that he can recognize individuals) or by a number of similar observers, in order to obtain an average evaluation for each object. On the assumption that the observers are able to satisfy the objective of equal intervals, the pile values may be numbered also with equal differences. By various checks it is possible to determine whether the observers have succeeded in their attempt to keep the steps equal. If constant errors creep in, they can be allowed for and corrections can be made in the pile values.

Fractionation Methods.—With the fractionation methods, the observer is usually presented with one standard stimulus at a time with the instruction to adjust the second, or to find a second, that appears to him to be half as great. The assumption is that he is bisecting the interval between zero and the standard magnitude. At times, he may be given a second standard stimulus very close to zero (zero being a liminal or threshold stimulus) as a guide.

Let us use again the example of pitch of tones as related to wave frequency. If, on the average, the observer finds a tone H to be half as high in pitch as a tone K, we may assign the number 1 to tone H and the number 2 to tone K. We could assign any numbers we chose if they have the same ratio of 1 to 2. By doing similarly with other standards and by combining the results into a single consistent set of values, an entire scale can thus be constructed and related to frequency. Steven's *sone* scale (1936) for the measurement of loudness of tones is an example of a scale developed by this general procedure.

Such scales are ratio scales, for they allow an interpretation of the absolute ratios of psychological magnitudes. A psychological magnitude given the number 10 is twice as great as that given the number 5, and so on. In ratio scales, a genuine zero point is pre-

supposed, although it may not always be located. For, if one magnitude is twice another, the smaller magnitude lies midway between zero and the larger magnitude.

Method of Doubled Stimulus. A similar method, but one that reverses the type of judgment, is known as the method of doubled stimulus. Instead of obtaining a magnitude that appears to be half as great as another, the observer tries to find one that seems twice as great as another. It is easily seen that the ratio scale can be derived by this procedure, too. This method might well be used as one check upon the results by fractionation, and vice versa. As to any choice between the two, the one that gives the greater acuracy would be preferred.

The Constant-Sum Method.—A method recently proposed by Metfessel (1941) calls for the direct estimation of the ratio between the psychological magnitudes corresponding to two stimuli. Thus, the observed ratio is not confined to halves or doubles, as in the two methods just mentioned. The observer is expected to tell what particular ratio one impression has to another. One operation by which the judgment is made is to say how many points out of a total of 100 should be allotted to one of the two stimuli and how many to the other. With one pair of stimuli, for example, the judgment might be 60 points to the one and 40 to the other. The ratio is then 3 to 2; the first magnitude is judged 50 per cent greater than the other, or one and one-half times as great. The possibilities and limitations of this type of judgment and the validity and accuracy of the resulting scale values have not as yet been explored extensively. It would appear to have promise of extending psychological measurement of the ratio type considerably. A point to be emphasized at this time is that the method assumes the possibility of observing directly the size of a ratio of two psychological magnitudes and that this provides an operation which parallels the ratio property of numbers.

In Summary.—In psychophysics, we are primarily concerned with discovering how a given psychological variable (continuum) is related to a certain physical variable (continuum). The nature of such a functional dependence is important for the purposes of pre-

diction and the discovery of quantitative laws. Fechner's law is only one of a number of psychophysical laws.

This goal makes it necessary to measure quantities on psychological scales. A few illustrations have been given to show how, by certain experimental operations, it is possible to achieve psychological evaluations on interval scales and even ratio scales. It is necessary to assume that human observers can compare and judge intervals and also ratios of experiences. The internal consistency of the results is the experimental proof of the validity of the method and of the fact that equal intervals and ratios are actually achieved. Proof is lacking that such results justify all the mathematical operations possible with the number system. These measurement procedures, like most others, must be evaluated in terms of what they do for us and of how much we can depend upon them to give concordant and meaningful results.

We have not given attention to all the psychophysical methods here, since many of them do not lend themselves to the scaling of psychological magnitudes. Some of the methods used next for illustrations may be utilized in psychophysical studies. They are called "scaling" methods, but the distinction between them and the psychophysical methods is very thin in places.

Scaling Methods

For many psychological variables no corresponding physical measure of the stimulus is available. In other instances we are not interested in the psychophysical problem of relating response values to stimulus values. The interest is then directed toward the psychological measurement only. The measurement, of course, is not an end in itself; it is made for some useful purpose—a scientific investigation or the solution of a practical problem.

The methods previously described can be used for scaling purposes, but there are others that are ordinarily more convenient, although none (with the possible exception of rating-scale methods) enables us to achieve ratio scales in the sense of directly observed ratios. By applying special treatment to the data, based upon certain rational considerations, we can often arrive at values on interval

scales. The methods to be mentioned here (there are others) include *rank order, paired comparisons*, and *rating methods*.

Rank-Order Methods.—The rank-order, or order-of-merit method, has long been a popular procedure for scaling psychological stimuli. In its simple form, each of a group of judges arranges several stimuli in order of increasing excellence, or quantity, or preference, with respect to the psychological variable under consideration. The stimuli may be art objects of a particular kind, specimens of handwriting, or a group of employees. If all we want in the way of a final answer about the psychological values of the objects is a consensus of the judges as to their rank order, we can average the ranks assigned by all judges to each object. A median is the most appropriate kind of average for this purpose. The resulting medians indicate nothing more than rank positions; they are ordinal values. The sizes of the intervals separating the scale values are not determined. Any numbers could be substituted for the objects so long as the rank order of the numbers corresponds with the rank order of the objects. Even letters of the alphabet could be used, since they possess the property of order.

Several procedures have been adopted in the past in the attempt to convert rank-order judgments into values on interval scales. Each procedure rests upon the basis of certain assumptions about the data. For example, one method assumes that the objects have psychological values such that a frequency distribution of those values is normal. This assumption has the backing of much experience, if the objects are living organisms or the products of living organisms. If the larger population from which the objects are selected at random is normally distributed on the continuum in question, samples will also tend to be normally distributed.

Consider the ten objects, A to J, in Figure 11.2 I, and their scale values which we will assume to be known. We cannot say that the ten objects are normally distributed, but they tend to bunch closer together near the center of the range and to be farther apart at the extremes, in consistency with one important principle of a normal distribution. We ask one observer to place the objects in rank order, and his powers of discrimination are so good that he can

do so without error To the ranked objects we assign numbers 1 to 10, 10 being given to the highest-ranking object. This operation is illustrated in Figure 11.2 II. Note how the ranking process, which gives equal unitary intervals between all adjacent pairs, does violence to the actual spacing of objects It gives the middle objects too much spacing, and the extreme ones too little, relatively.

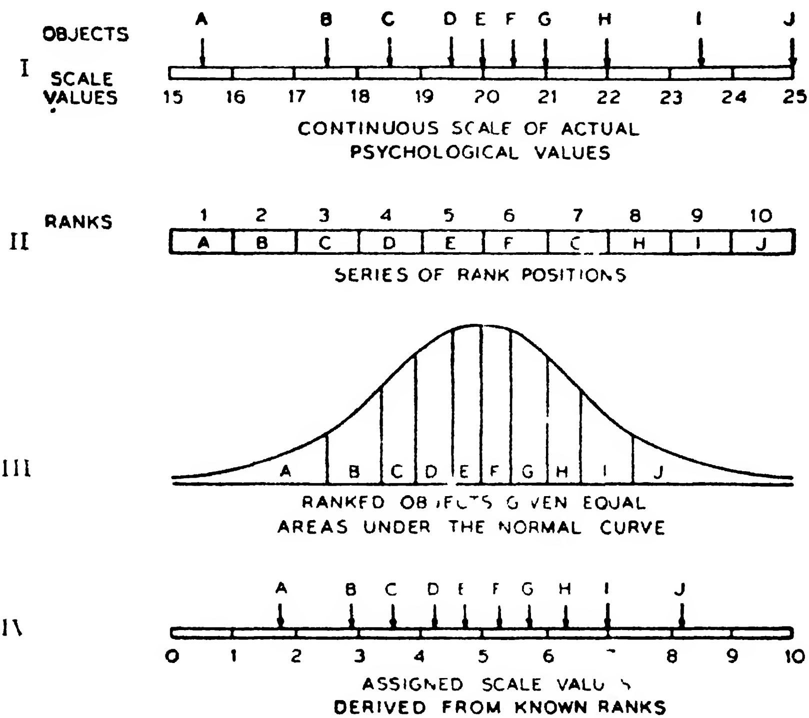

FIG. 11.2 Illustration of the scaling of objects by judgments of rank order and the conversion of ranks into quasi interval scale values by making the assumption of normal distribution

If we assume that the ten objects are normally distributed, and if we maintain the rank positions assigned to them by the same observer, the objects would occupy spaces as shown in Figure 11.2 III In II, each object was given equal space in the form of a rectangle and this implies that it occupies the same amount of linear distance on the numerical scale or base line as any other The entire distribution is therefore assumed to be rectangular In III, each

object is given equal space, but because the spaces under the normal curve are higher near the middle, objects near the middle therefore extend over much less distance on the base line, which is the scale of measurement. Since the scale value for each object approaches a point rather than an interval, however, we want a single value to represent it. This value is at the point which divides each object's area into two equal parts. These values are shown in Figure 11.2 IV.

The precise steps by which we start with a single observed rank position for an object and from it derive a scale value as represented in Figure 11.2 IV, need not concern us here. The rigorous mathematical relationships between certain areas under the normal distribution curve and base-line positions are the basis for this conversion process. It is important here merely to realize that by assuming that a certain mathematical description may be given to the relation between the actual scale value and an observer's rank for the object, we are able to derive scale values which approach, although they do not necessarily equal the actual values. The resulting scale is appropriately called a *rational scale*.

Compare the spacing of the objects in Figure 11.2 I and Figure 11.2 IV, and it will be noted that there is not a coincidence in the relative spacings, but they are much closer in agreement than are the ranks in Figure 11.2 II with either of them. The averaging of such rational scale values derived from different observers should tend to iron out some of the errors that are likely to beset the values obtained from a single observer. Although the averages may not have the complete properties of a good interval scale, at least the gross distortions produced by ranking (as in going from I to II in Figure 11.2) are counteracted. In many instances the investigator will have enough confidence in the approach to an interval scaling that he will use the data in computing a Pearsonian r.

There are still other rational methods for converting rank-order information into values of objects on interval, or near-interval scales. Some of the more common ones extract from rank orders inferences about the proportion of the time each stimulus is judged greater than every other stimulus. The principle of this scaling method is

best explained by reference to scaling in the method of paired comparisons, which this procedure for dealing with ranks most closely resembles.

Paired Comparisons. In the typical application of paired comparisons, the stimuli are presented to each judge in all possible combinations of two, with certain details of presentation order arranged to minimize constant errors. The number of times each stimulus is preferred to every other one is determined.

Since each stimulus has an equal number of opportunities to be preferred (or judged greater), the total number of preferences would serve as a satisfactory indicator of its rank position among all the stimuli compared. If all one needs to know about the stimuli is their over-all rank order, these numbers would serve the purpose of ordinal scaling. New rank numbers could be substituted for the obtained frequencies if that is desired. If one goes to the experimental trouble of collecting paired-comparisons data, however, one might well attempt to extract from the same information something more nearly approaching interval-scale values, with a little more calculation.

Thurstone (1927) devised a method of treating the results from paired-comparisons judgments to derive scale values on an interval scale. The foundation for this method is Thurstone's *law of comparative judgment*. Several concepts must be explained before the law can be understood.

Thurstone assumed that any stimulus above the threshold gives rise to a psychological reaction within the organism which is called a "discriminal process." A particular physical stimulus R_1 will not necessarily always give rise to a particular reaction, S_1, but on successive occasions may result in a spread of psychological reactions varying in strength, and distributing themselves normally about a most frequently aroused value called the "modal discriminal process." The distribution of such discriminal processes yields a standard deviation, which measures the discriminal dispersion. The assumption is made that every discriminal process is the modal process for some corresponding stimulus magnitude and that for every physical magnitude or stimulus value, there is a corresponding modal discriminal process.

Let us next think of a stimulus R_j and its corresponding modal discriminal process, S_j, and another stimulus, R_k, and its modal discriminal process, S_k. The discriminal dispersions of these two distributions are measured by σ_j and σ_k, respectively. Thurstone's law of comparative judgment is then stated as follows:

$$S_j - S_k = z_{jk} \sqrt{\sigma_j^2 + \sigma_k^2 - 2r_{jk}\sigma_j\sigma_k}$$

where S_j and S_k are the scale values for the modal discriminal processes, z_{jk} is the deviation from the mean of a unit normal distribution corresponding to the experimentally obtained proportion of judgments, $R_j > R_k$, σ_j and σ_k are the discriminal dispersions of the respective discriminal-processes distributions, and r_{jk} is the correlation between the scale values of the discriminal processes.

In this form, the law is not practically usable for obtaining scale values for stimuli judged by the paired-comparisons technique. The correlation between discriminal processes for each stimulus pair is not known, nor are the discriminal dispersions available at this time. Thurstone made several assumptions which allowed a transformation of the law into a form for practical use. For the simplest case, it was assumed that: (1) the law holds for many individuals making one judgment for a pair of stimuli as well as for one individual making many judgments; (2) the correlation between discriminal processes is zero; and (3) the discriminal dispersions are equal. The law then reduces to:

$$S_j - S_k = z_{jk} \sqrt{2}$$

In this form, the law is easily used to solve for the difference between any two scale values, for the proportion of judgments $R_j > R_k$ is known from the paired-comparisons judgments, and this proportion can be used to find z_{jk} from normal-curve tables. Having estimated differences among all pairs of objects, we can use them to rank-order those objects on the psychological scale, and, by averaging, to estimate scale distances between adjacent pairs. Having these, by successive additions we can assign scale numbers to the objects. The unit of the scale is the standard deviation of the dispersion of any one of the stimuli, since they are all assumed to be equal. Tests

of internal consistency of the obtained scale values can be made to determine whether the assumptions mentioned above—equal discriminal dispersions and zero correlations—are sound.

The units on scales of this type are supposed to be equal, i.e., an interval scale is developed, but it should be emphasized that equal units of this kind are not the same as those obtained with fundamental measurement. There is no experimental verification of the laws of addition, nor even an experimental interpretation of equality of differences between scale values. Gulliksen (1946) has pointed out that, operationally, the value obtained by subtracting two scale values derived from the law of comparative judgment in its simplest form actually represents a certain proportion of judgments and not a magnitude such as in fundamental measurement

This does not condemn the type of scaling achieved with the law of comparative judgment, but merely places a limitation upon the interpretation of the results obtained when it is applied. Care must be exercised in interpreting the numbers assigned by such methods so that inferences will not be made which would be justified only by a process of fundamental measurement. The information obtainable by the law of comparative judgment is useful, and gives a method of measurement which goes beyond ordinal measurement. The meaning of the numbers assigned, however, must always be considered in the light of the operations and assumptions underlying the method.

Rating Scale Methods.—The principle underlying almost all the rating-scale methods is the definition of a presumably unique psychological continuum upon which the judges are to locate individual samples Usually, descriptive phrases are supplied to indicate important positions along this continuum by which the judge can accurately locate the sample rated. Such methods have been particularly popular in the quantitative judgment of character traits and other personality traits.

Where the important points along the psychological continuum are defined by descriptive phrases with attached numbers or letters, a *numerical rating scale* or *scale of values* is achieved. Where the important points along the continuum represent a scale established

by some other scaling method, such as by paired comparisons, a *scale of standards* is obtained This general procedure is similar to almost all types of measurement, for after a scale is set up, numbers are assigned to other magnitudes by finding a member of the standard scale to which an unknown magnitude is equal Another common scale, called the *graphic rating scale*, requires the judge to check a point along the continuum represented by a straight line, with descriptive terms applied at various points from one end to the other Other variations of these methods have been devised

If the observer is required to make his judgments in terms of an absolute numerical scale, keeping in mind the ratio properties of numbers as he assigns them to objects it could be argued that the resulting evaluations of objects are on a ratio scale Probably no rating scales in use have attempted to achieve this goal If the judge has, to guide him some descriptive categories that have previously been scaled on an interval scale as by paired comparisons for example, it may be argued that such ratings are evaluations on an interval scale The fact is that in practice most rating procedures do not attempt even this much refinement consequently the numerical values resulting, strictly speaking are ordinal There are operations similar to those described in connection with ranking methods and paired comparisons for converting ratings into evaluations of objects on scales that satisfactorily approach interval scale values

In Summary.– In this section some of the most common psychological scaling methods have been briefly described and related to the common types of measurement scale The least that any of them does is to yield values for objects on ordinal scales in which relative scale positions are indicated by assigned numbers Only in the case of methods, under optimal conditions, in which the observer performs the operation of reporting on ratios can we say that ratio-scale values have been achieved By certain rational methods, i e, by making certain assumptions, such as normality of distribution, we can "correct" original reports of observations (judgments), or convert them into values which partake of interval-scale properties. The law of comparative judgment is one such rational basis for scaling proportions to appropriate interval positions

Mental Testing

In measuring human ability and achievement variables, the mental test has become the medium through which numerical description is applied. Indeed, the mental test has become so popular and widespread that it represents perhaps the most common type of psychological measurement.

Considerable criticism has been directed at mental testing in the past by individuals both within and without the ranks of psychologists. Some of these criticisms have been directed at basic philosophy behind mental testing, and others have claimed that mental testing is not measurement at all, since it does not satisfy the logical criteria underlying fundamental measurement.

At the outset, it can be freely admitted that mental testing does not at present provide methods of assigning numbers to psychological variables, such as mental abilities, that are comparable with fundamental measurement in physics. It has already been pointed out, however, that the failure of psychological measurement to satisfy all the logical criteria of complete measurement, e.g., the rules of addition, does not necessarily mean that numerical description of psychological events is impossible. Some of the methods already discussed point to the possibility of securing scales of measurement in psychology with operational significance attached to units and ratios, even though such units and ratios represent different concepts from those achieved with fundamental measurement.[7] A matter of great importance, however, is the task of clarifying the significance of the numbers that are assigned by means of mental tests. What properties, if any, are properly attributed to mental test scores beyond the rank-order level? In this discussion we will generally have in mind the type of test in which the score is some function of the number of correct responses.

Equal Units and Mental Testing. One of the most desirable properties in mental-test scales is that of equal units along the measurement continuum. The importance of this property is due to the

[7] Not all ratio scales, as the term has been broadly used in this chapter, exhibit fundamental measurement. There are ways in which an operation of equating ratios can be demonstrated experimentally when operations of addition cannot.

demand for a constant unit in the computation of means and standard deviations which underlie so many of the important statistical devices for drawing experimental conclusions and predicting human adjustment. For this reason, much of the effort by mental testers has been devoted to developing mental tests which would yield scales with equal units.

One well-known technique for achieving interval scaling is the normalized standard scale. The assumption is made that test scores are actually normally distributed for a particular test variable with an adequate population. The proportion of individuals in a sample falling

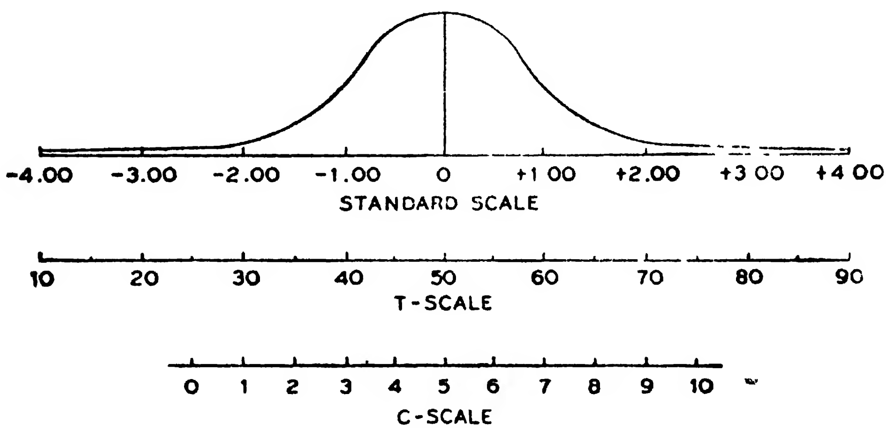

FIG. 11.3. Illustration showing equivalent values on the standard scale, T scale, and C scale.

below a certain test score can be used to establish a point for that score on a curve of normal distribution. All scores are thus converted into a scale in which the mean of the distribution is zero and the unit is equal to the standard deviation. The first scale in Figure 11.3 is such a scale, shown extending from -4.0σ to $+4.0\sigma$. It can be extended further at both ends, but for the usual population with which we have to deal in practice, three units either way from the mean take in practically all cases. In the use of the standard scale, it is usually assumed that the standard deviation is a unit representing equal increments of the psychological variable throughout the scale.

There are a number of variations of the standard scale, most of which are based upon similar assumptions. One of these is the well-

known T scale, in which the mean is arbitrarily placed at 50 and the unit is one-tenth of a standard deviation. Another is the less well-known C scale (Guilford, 1950), in which the mean is 5.0 and the unit is one-half a standard deviation Both of these scales are illustrated in Figure 11.3. The C scale extends from -2.75σ to $+2.75\sigma$, which includes over 99 per cent of most samples.

Useful and meaningful as these types of scaling are, it should be clearly realized that there is no proof that interval scales have been achieved in such manner. For one reason, we cannot prove that the population distribution is normal The use of the normal distribution pattern can be defended, at best, on the grounds of convenience in the use of a familiar concept A more fundamental reason is that we do not know that any particular scale, either of raw scores or of scaled scores, is correlated with the psychological variable in a one-to-one fashion. The human trait is a variable, apart from the scores by which we attempt to measure it The scores, raw or scaled, represent still another variable How it is related to the trait variable we do not know There is no assurance that a difference of one test item correct will represent an equal difference all along the scale with respect to the underlying psychological variable. Nor is there any assurance that the standard-deviation unit under the normal curve will represent equivalent intervals of the underlying psychological variable.

In order to determine whether any set of units on the test-variable scale represents equivalent units on the continuum of the underlying variable, it would be necessary to establish the nature of the functional relationship between the two In order to know this functional relationship, some independent measure of the psychological variable would be needed Since this result is impossible of attainment with the present state of knowledge, it seems unlikely that mental-test scales will be proved to contain equal units, in any other than a defined, or assumed, sense The same conclusion can be applied to the determination of absolute ratios of magnitudes of psychological quantity in the realm of traits.

This analysis forces us to the conclusion that interval scales in mental testing must be interpreted in terms of certain assumptions

and operations of a statistical nature rather than in terms of an experimental proof of equal psychological units along the scale of measurement or in terms of observational operations. The significance of numbers assigned to mental-test results is thus limited somewhat in scope, but such numerical description is useful and certainly may be employed as long as its limitations are kept in mind.

Mental-Age and IQ Scales.—Of a quite different type and almost as commonly used, are the familiar mental age and IQ scales. Since the basic "unit" for mental age seems, at first thought, to be a time unit, and physical time is measured on a fundamental type of scale, it would appear that here is an ideal psychological-measurement scale. Unfortunately such is not the case. The use of time units, like the use of other physical stimulus equivalents in psychophysics, by no means yields fundamental measurements. There is no well-established knowledge of the functional relationship between mental growth on the one hand and the chronological age scale, on the other. The least that can be said of the mental-age scale is that it is an ordinal one. How far it approaches an interval type of scale is unknown.

The IQ, being a derived index, based upon chronological age, which is on an interval scale (it would be a ratio scale if the zero point were located at the time of conception rather than at the time of birth), and upon mental age, whose scale character is not known, has uncertain status as a scale. The fact that distributions of IQ's tend to be normal (with some assistance by those who construct age-scale instruments) is reassuring to some. There is no proof, however, that a range of 10 IQ points on one part of the scale is equivalent to 10 IQ points on any other part as indicators of variations of whatever the IQ scale represents psychologically. In the application of statistical operations, the IQ scale is almost always treated as an interval scale, and without question. The justification for this, like that for the use of raw point scores or scaled scores, must rest on grounds other than the usual ones for measurement.

Mental Testing and Validity.—In reality, the final criterion by which the value of a mental test can be judged is one of validity.

That is, can the test predict the success of an individual, or of a group of individuals, in some type of adjustment?

Suppose the scores from a certain test are treated statistically by computing means, standard deviations, coefficients of correlation, and multiple regression equations, all of which presume equal units, and that such scores can be used successfully to predict in a continuous fashion which individuals will succeed and which ones will fail in a particular adjustment. While the method may not yield perfect prediction, the results are better than prediction without the knowledge of test scores. This state of affairs is sufficient justification for the use of the method, even though a complete logical foundation cannot be experimentally established.

In other words, even if equal units cannot be proved for a mental-test scale, the result obtained by proceeding without such proof is one of successful prediction, so there is little point in citing the lack of such proof as sufficient reason for abandoning the procedure. This does not mean that the attempt need not be made to devise other means of prediction with better logical foundation. If such methods were found, and they failed to improve prediction, however, there would be insufficient justification for abandoning the old procedure. The ultimate goal for a mental test is one of describing and predicting human behavior. To the extent that the test is successful in achieving this goal, it is useless to assert that the method is of no value because it does not conform to certain logical criteria which have been established for fundamental measurement. The justification by means of demonstrating practical validity is an empirical one, and therefore acceptable, even if it represents a departure from the use of conventional logical criteria for measurement. Care must be exercised, however, not to assign unjustified meanings to numbers obtained in mental testing.

Improving Test Validity.—The road to improving mental measurement, then, will probably lie in the direction of an empirical attack upon the problems of increasing test validity rather than through attempting to devise a means of meeting the logical criteria underlying fundamental measurement. Psychologists working in the mental-test field have been doing just this almost from the begin-

ning. Many improvements have resulted which would certainly have been delayed had attention been concentrated solely on fulfilling logical requirements for measurement.

The recent development of factor analysis has done much to forward the progress of mental testing toward the goal of improving practical validity. In fact, factorial theory and methods have resulted in a revision of the concept of validity, itself. Prior to factor analysis, the evaluation of a test was made in terms of its correlation with some practical criterion, i.e., its practical validity, or by correlation with some well-known test. The difficulties with these methods increase considerably as tests are combined in batteries for the prediction of some practical criterion.

When tests, which individually correlate highly with some criterion, are combined to predict that criterion by means of multiple regression equations, it is frequently found that very little is added to the predictive efficiency over what could be accomplished with just one test. This condition results from the fact that the tests often are measuring virtually the same variables. Intercorrelations of the tests themselves will reveal this situation to some extent, but it is difficult to know just what will happen when several tests are combined.

Factor Analysis and Measurement.—Factor analysis has provided a method of escape from this disturbing situation. It is possible to determine by factor-analysis methods just what variables are being measured by a test and what variables need to be measured to predict a particular criterion. Then, tests can be developed to measure the variables that are needed. This procedure eliminates much of the guesswork from prediction by means of test batteries.

From the application of these factor techniques, numerous variables can be isolated which represent more or less independent abilities, such as verbal, numerical, perceptual-speed, and reasoning abilities, and which are not correlated with each other to any appreciable extent. A new type of validity, called "factorial validity," has been conceived which represents the extent to which a particular test measures one of these unique abilities, or *factors*, as they are called.

The ultimate aim of such factor-analysis procedures is to facilitate the development of tests which measure one and only one of

these factors. When enough tests of this type, called "pure tests," are developed to measure all the necessary variables to account for human variability, they may be combined in various groups to measure and predict almost any conceivable practical criterion. It is only necessary to determine what abilities are involved in a particular criterion to know what tests should be used to predict it.

The research now being carried on through factor analysis and many other lines of development are gradually increasing the effectiveness of mental tests as means of measuring important psychological variables. This kind of measurement is unique among scientific procedures, but it is nevertheless well established upon the grounds of predictive effectiveness. If the methods in psychology for achieving accurate prediction are different from those of the more exact sciences, as they are, an interesting fact has been noted, but that fact does not constitute an adequate basis for criticism. Psychological-measurement procedures will stand or fall on the basis of their success in predicting and describing human behavior rather than their success in conforming to a set of criteria designed for other sciences.

In Summary.—Common numerical scales on which the results of mental tests are indicated, whether they are in the form of raw scores, scaled scores, mental ages, or IQ's, must be regarded, in all strictness, as ordinal scales, in so far as any proof of an experimental sort is concerned. Scaled scores often rest upon the assumption of a normal distribution of the population with respect to the psychological variable measured; an untestable assumption. Mental age is merely a convenient reference to a physical time scale for meaningful interpretation, but without the demonstration as to the kind of relation of mental growth to chronological age.

Yet, the common statistical procedures that require interval measurements as a logical justification are applied to mental-test data on every hand. The unwritten law of the psychologist seems to be that he will apply the statistics without thought of logical justification, and if the outcome is meaningful and useful, the risk is well taken. The validity of the results, in terms of prediction of human

adjustment, may be taken as justification of the practice, if not of the assumption of equal-unit scales.

The problem of test validity is undergoing marked reinterpretation and the procedures for increasing the practical validity of tests is being greatly facilitated by the application of factor analysis. By leading to better knowledge of the underlying psychological variables, factor analysis is helping to decide *what* to measure. This is fully as important, if not more so, than knowing *how* to measure.

Concluding Statement

In this chapter an attempt has been made to give a clearer orientation than has probably prevailed before, of the logical problems underlying psychological measurement and how they have been solved, wittingly, or unwittingly, to the present time. There is no gainsaying the desirability of utilizing the rational number system as a most accurate and convenient language for scientific descriptions. The things that can be done with numbers, under the rules laid down by mathematics, are worth the great efforts that psychologists have put forth to use them. There is evident craving on the part of the psychologist to profit from the advantages that numerical descriptions have to offer. His desire has outrun his caution in many instances, and he has often proceeded to measure when there are no good logical justifications for the operation.

It has been pointed out in preceding pages that we may apply numbers as descriptive symbols and ideas only when and in so far as the structure of the phenomena in a certain area of experience is similar to the structure of the number system. Properties of the observed experience must parallel the properties of the number system. For example, one event must be greater than, equal to, or less than another in some defined respect known as a variable. It is also important that we be able to equate intervals between pairs of events in order to justify assignment to the pairs numbers whose differences are equal. It is desirable that the ratios of observed quantities be equal when we assign to them pairs of numbers whose ratios are arithmetically equal. For complete measurement, it is important that we can actually add one event to another and come out with a

composite that can be predicted from the numbers assigned to the single events. This requirement is essential for full justification of all numerical operations applied to measurements. It is fully satisfied only in a few instances in physical measurements, as in evaluating the lengths of lines.

Critics of psychological measurement have usually taken the stand that measurements are complete or they are not measurements at all. There are no halfway measurements. The position taken in this chapter is that there are all degrees of satisfaction of the requirements for measurement; different extents to which the structure of events may be shown to duplicate the structure of the number system. Measurement is here conceived as an operational matter. Certain experimental operations possible with certain psychological events have the properties of rank order only; certain others also permit the application of equal intervals; and still others achieve the property of equal ratios. The more of the number properties that can be demonstrably satisfied, the more we can do with the numbers assigned, in the way of statistical treatments and the application of mathematical equations. It is expected that improvements in experimental operations will justify an expanding use of measurement in psychology.

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CHAPTER 12

INTELLIGENCE

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THE PROBLEM OF INTELLIGENCE

"What is intelligence?" This question, in just this form, was the subject of lively discussion by psychologists in the period from about 1900 to 1925. The question is older than the science of psychology, for it was discussed by philosophers and biologists before the emergence of psychology as an independent discipline

The first psychologists to discuss the nature of intelligence continued what might be called the philosophical method, i.e , they constructed definitions on the basis of logic and intuition. During the same period the first successful mental tests were being constructed. There was not always a clear connection between a given psychologist's proposals for the definition of intelligence and his proposals for the measurement of intelligence

As time went on, psychologists became increasingly disposed to discuss and analyze their data, the results of tests and experiments, rather than to discuss logical and intuitive analyses apart from empirical data. This trend, of course, has been evident in other fields as well as in relation to intelligence.

When we confine our attention to actual data, we find that the problem of the nature of intelligence becomes intertwined with many other problems: What is the course of intellectual development in normal children, in those above average, in those below average? To what extent is the development of intelligence due to hereditary factors and to what extent to environmental factors? How shall we measure intelligence as distinct from achievement?

Around 1920 to 1930 a number of large research projects were undertaken to supply final and definitive answers to such questions.

Analysis and criticism of the results of those projects probably convinced all concerned that no one question could be completely answered without finding out a good deal about a number of closely related, and equally difficult, questions.

Today probably most of those actively engaged in research in the field of intelligence would be embarrassed to answer a question as broadly phrased as, "What is intelligence?" Instead, they address their research to more specific questions: What is the nature of the abilities into which the adult human mind is organized? How does mental organization change with age? How broad, in respects other than age, is the group for whom our picture of mental organization will hold?

In the present chapter we will review briefly the philosophical discussions of the nature of intelligence and more extensively contemporary research on the organization of intelligence. So far as possible we will avoid such collateral questions as the relative influences of heredity and environment. There is, however, one collateral question which we cannot hope to avoid: To what extent do our data give us information about the human mind, and to what extent do they give us information merely about the tests we happen to have constructed and about the statistical methods we use to analyze the data? And we shall also ask: To what extent have our theories about the nature of intelligence helped us to measure intelligence? The methodological orientation of this chapter is partly a reflection of published research and partly a reflection of the writer's predilections

The Philosophical Solutions

Many psychologists have proposed definitions of intelligence. There is legitimate question whether any of these definitions has influenced modern psychology more than the others; therefore, the selection of definitions will be based on the importance of the authors in the history of mental testing Grouping of the definitions into types and the discussion of the difficulties inherent in each type will follow the argument of Spearman in his classical *Abilities of Man* (1927).

Spearman divided the philosophical solutions into three main types: those which gave some definition of *general intelligence*; those

which gave separate definitions for each of several *formal faculties,* which together comprise intelligence; and those which defined intelligence as the *general level* or *average* of a very large number of specific (but unspecified) abilities.

General Intelligence.—Ebbinghaus (1897) defined intelligence as the power of combination, and, in accordance with this definition, proposed a sentence-completion test as a measure of intelligence. Tests of the sentence-completion type are still in frequent use. Terman (1921) defined intelligence as the power to think abstractly; Thorndike (1921) as "the power of good responses from the point of view of truth", Thurstone (1921) as "that which can be judged by the degree of incompleteness of the alternatives in the trial and error life of the individual." Other typical definitions have stressed learning ability and the power of adaptation to new situations.

Spearman criticized these definitions of general intelligence on a number of grounds.

(1) Many of them make valuable statements about intelligence, but they do not enable us to tell in which performances intelligence is to be found. How about memory tests? Tests of "imagination"? Perceptual tests? Exclusively verbal tests? Psychologists disagree about the inclusion of each of these types of tests, and the proposed definitions do not resolve this disagreement.

(2) The various accepted tests of general intelligence do not all test exactly the same thing, nor do they test entirely independent things. Rather they exhibit an intermediate degree of correlation. Our theory of intelligence must account for the fact that there is fairly high, but not perfect, correlation between these measures of intelligence.

(3) No evidence was offered that the "general intelligence" of any of the definitions existed as a single, measurable faculty. In the face of that argument, some of the proponents of general intelligence suggested the division of that ability into several component abilities, each of which, they said, should be measured separately.

Formal Faculties.—Although modern psychology congratulates itself on having advanced beyond the stage of describing the human mind in terms of formal faculties, Spearman wrote, the doctrine of

formal faculties continues to be influential, however disguised. He lists the following faculties, all to be found in ancient psychology, and frequently mentioned by modern psychologists as components of human ability: memory, intellect, sensory perception, imagination, attention, movement, language. There is considerable variation in terminology. Intellect, for example, is taken as equivalent to abstraction, ideation, and thought; movement appears under such names as motor control, manual dexterity, and kinesthetic discrimination.

Binet said a great many things on the subject of intelligence, and which of his remarks he would like to have us consider as a definition of intelligence, no one can say. A frequently quoted passage states, "Comprehension, invention, direction, and censorship; intelligence lies in these four words" (Binet, 1910, p. 118). Terman summarized Binet's conception of intelligence in terms of "(1) its tendency to take and maintain a definite direction; (2) the capacity to make adaptations for the purpose of attaining a desired end; and (3) the power of auto-criticism" (Terman, 1916, p. 45).

Spearman pointed out that the better a test of general intelligence, such as Binet's test, measured one such faculty, the less well it must measure the others, yet Binet never proposed measuring these faculties separately. In defense of Binet, one should note that both his definition and his tests emphasize the "higher mental processes," as opposed to the tests of simple sensory and motor processes which most previous psychologists had hoped would yield measures of intelligence.

As in the case of general intelligence, there was in 1927 no evidence that the various proposed faculties existed as measurable, functional entities. "Even when a faculty is quite acceptable in the sense of indicating a mere *class* of mental abilities, it cannot be taken without further evidence as implying any *correlation* between these, as would be indispensable for the purpose of measuring them all by any single test" (Spearman, 1927, p. 38).

General Level or Average.— Concerning correlational studies of mental tests, Thorndike wrote,

> "A table of the known degrees of relationship would abundantly confirm the statement that the mind must be regarded not

as a functional unit, nor even as a collection of a few general faculties which work irrespective of particular material, but rather as a multitude of functions each of which involves content as well as form The mind is really but the sum total of an individual's feelings and acts, of the connections between outside events and his responses thereto, and of the possibilities of having such feelings, acts and connections It is only for convenience that we call one man more learned than another instead of giving concrete lists of the information possessed by each and striking averages from all the particulars, that we call one man more rational than another instead of comparing two series of rational performances" (Thorndike, 1914, pp 366-367)

This view of intelligence survived in the widely held belief that what is measured by a test of general intelligence is a "general level" or "average" or "sample" of the individual's abilities Spearman argued that the definition of intelligence in any of these terms was inadmissible

Taking first the term "average," Spearman showed four "violated postulates," or four respects in which the score on a mental test could not be considered a true average

(1) In order to compute a true average, we must first settle the domain over which the average extends This has not been done for intelligence For example, there is disagreement over the inclusion of tests of memory, perception, and motor abilities

(2) In computing an average, the cases to be averaged should be somehow equivalent or comparable "It would be ridiculous to try to take the average expenditure for a number of periods, if some of these happen to be weeks, some months and others of unknown duration" (Spearman, 1927, pp 63-64) Into how many sub-abilities should we divide judgment, how many different kinds of memory are there, how many kinds of imagination, and so on? The problem appears insoluble

(3) A true average cannot be obtained where the same items are introduced repeatedly In obtaining average weekly expenditure, the week before Christmas should not be included several times! Very often the same ability appears to be included in a number of items or sub-tests

(4) A true average must include every item in the relevant domain. "What would be the use of a man averaging his expenditure, if those weeks were omitted in which he paid his rent? Yet no one can seriously believe that any scale of tests is able to escape making grave omissions" (Spearman, 1927, p. 64).

Probably most testers, however, conceive of their tests as measuring a sample of all mental abilities rather than an average. But a representative sample must meet requirements similar to those of an average. The domain to be sampled must be defined unequivocally, and each case within that domain must have an equal chance of being drawn. In constructing tests, most commonly a large number of items are tried and those retained which correlate highest with all the rest. This procedure accentuates any bias in the original sample of items. "In order to obtain a genuine sample, one carefully retains, and even adds to, the sorts which were at first little represented, and which therefore tend to be *least* correlated with, the remainder" (Spearman, 1927, p. 70).

To Spearman's criticisms of the philosophical solutions to the problem of intelligence, one general criticism may be added. So long as we are willing to settle the problem of the nature of intelligence by constructing logical definitions, there will be indefinitely many definitions to choose from, and there will be no widely accepted criterion for choosing among them.

The Pragmatic Solution

If we conceive of the human mind in terms of innumerable specific abilities and admit that we cannot truly average or sample these abilities, there is still an alternative basis for testing intelligence. We may say, "What I mean by intelligence is what this test measures." Although Woodworth (1948, pp. 117-118) and others have called this statement an example of operational definition, it was current and influential in psychology prior to the time of self-conscious "operationism."

The statement seems to have been originated by Boring in a reply to a series of articles criticizing the mental test movement, written by Walter Lippman and published in the *New Republic*.[3] Boring

(1923) wrote "Intelligence is what the tests test. This is a narrow definition, but it is the only point of departure for a rigorous discussion of the tests. It would be better if the psychologists could have used some other and more technical term, since the ordinary connotation of intelligence is much broader. The damage is done, however, and no harm need result if we but remember that measurable intelligence is simply what the tests of intelligence test, until further scientific observation allows us to extend the definition."

Thorndike provided an example of the application of this view when he called what is measured by a well known test he has devised 'Intellect CAVD.' The letters CAVD refer to the four kinds of tasks included in the test, namely completion, arithmetic problems, vocabulary, and following directions. For any ability, Thorndike said, 'the measurement of the ability is essentially an inventory. We can satisfactorily define the ability only by a list of the products which it produces, the tasks which it achieves' (Thorndike, 1927, p. 477).

A less radical expression of the pragmatic view is that of Terman (1916, pp. 42-44), who argued that intelligence can properly be measured prior to its definition, just as electricity was successfully and accurately measured at a time when its real nature was erroneously conceived. Spearman's (1927, p. 16) answer to Terman was that while we need not know the real nature of intelligence, we must at least know in which mental operations it can be found, and this is precisely the point at issue. The psychologists are in the position of a physicist trying to measure an electric current without knowing which of several galvanometers are really in circuit with it.

Woodworth's (1948, p. 118) criticism of the pragmatic view is undoubtedly shared by a majority of psychologists. If intelligence is no more than what this test measures, then the whole problem of validity is sidestepped and there is left no criterion for the value of a test but its reliability.

Two avowed operationists have also criticized what is here called the pragmatic solution. Feigl (1945) wrote in this connection: 'Quantitative concepts . . . are usually the product of long labors of adjustment by repeated redefinition. Thermometers or intelligence tests did not arise in an historical vacuum. They were devised in a

context of problems that arose out of a background of previous qualitative and semi-quantitative knowledge."

Bridgman's (1945) comment on the pragmatic view was, "The assertion as it stands begs the question. The question-begging word is the humble 'what.' The assertion that the intelligence test tests a 'what' implies the repeated application of the test and the discovery that the results have the properties of a 'what.' It seems to me that the actual situation here is one of spiral approximation, as it so often is."

Like the philosophic definitions of intelligence, the pragmatic definition in terms of a particular test or a particular set of items is open to the objection that there are indefinitely many such definitions. We must look elsewhere for a criterion by means of which one such definition can be established as more valid than another.

The Factorial Solutions

Characteristics Common to All Factorial Solutions.—All methods of factor analysis begin with the assumption that an individual's score on a psychological test is the weighted sum of his scores on the separate mental "factors" or abilities which enter into performance on that test. A given factor may enter into the scores of several tests: in fact, its nature can be explored only if it does. The factor score is a characteristic of the individual and will have the same numerical value for that individual in all tests in which that factor occurs. The coefficients of different factors characterize the test and are the same for all individuals.

Algebraically, we may write the basic assumption of factor analysis as follows:

$$(1) \qquad X_{ij} = a_{1j}z_{i1} + a_{2j}z_{i2} + a_{3j}z_{i3} + \ldots + a_{rj}z_{ir} + s_{ij}.$$

In this equation X_{ij} refers to the standard score [1] of individual i on test j. The quantity z_{i1} refers to the score of individual i on the first factor, this score also being expressed in standard form. The coefficient a_{1j} refers to the "weight" or "loading" of the first factor in

[1] Standard score means a score which, for that group, has a mean of zero and a standard deviation of one.

test j; the larger the coefficient a_{kj}, the more important is factor k in determining the total score on test j. There are r factors. The term s_{ij} refers to the "specific factor," which is an elegant name for what is left over, the margin of error, when we have found the best possible values to substitute in the rest of the equation. The specific factor is itself supposed to be the sum of two factors, one representing the error of measurement of the test, and one representing one or more systematic factors not present in other tests in the battery. At this point one may well take warning that different books and articles on factor analysis present this basic assumption in a variety of notations.

The immediate purpose of factor analysis is to assign numerical values to the coefficients, the a's, in this equation. In order to accomplish this purpose one must have a sample of individuals who all take a battery of several tests involving more or less the same factors. The relationships between the scores on the various tests are then ascertained in terms of correlation coefficients, which can be presented in a table such as Table 12.1.

TABLE 12.1.—MATRIX OF CORRELATIONS

		Test						
		1	2	3	4	.	.	m
	1		r_{12}	r_{13}	r_{14}	.	.	r_{1m}
	2	r_{21}		r_{23}	r_{24}	.	.	r_{2m}
	.	.	.			.	.	.
Test	.	.		.		.	.	.
	.	.	.	.	.		.	.
	.	.	.	.	.	.		.
	m	r_{1m}	r_{2m}	r_{3m}	r_{4m}	.	.	

Since there is only one correlation between any given pair of tests, Table 12.1 is "symmetrical about the principal diagonal." The analysis cannot be carried out until some quantities are entered in the principal diagonal, but the entries will differ in the various methods of factor analysis. The data of factor analysis are con-

tained in the table of inter-correlations. Once the correlations are determined, the scores of individuals usually are not referred to again.

To determine the a's uniquely, additional restrictions must be placed on the values they can assume. These restrictions differ in different methods. They are justified in terms of mathematical or intuitive psychological considerations.

Having obtained the a's, one can now characterize each test in terms of how much of its variance is accounted for by factor 1, how much by factor 2, and so on through all the factors common to several tests. For each test there will remain some residual variance which represents the contribution of factors specific to that test.

So far the common factors have been designated by arbitrary numbers or letters. Now each factor can be examined in terms of which tests it enters into strongly, weakly, not at all, positively, and negatively. By an intuitive judgment concerning the nature of the performances involved in each test, a notion is obtained of the nature of the factors. Often names or meaningful symbols will then be assigned to the factors.

Most factorial studies go no farther than this: a weighting or loading of each test on each factor is determined, and a name is assigned to each factor on a more or less intuitive basis. A psychologist with a practical turn of mind may well question at this point whether the game has been worth the candle. All this computational labor—and it is usually a lot—to obtain intuitive names for hypothetical factors of mind.

The claim is sometimes made that factor analysis will help put vocational guidance on a truly scientific basis. Referring to the basic assumption of factor analysis, the left-hand side of all such equations—for each analysis the number of equations will be the number of people times the number of tests--refers to the scores of individuals on tests, and these of course are known at the start. On the right-hand side the a's are now known; therefore one can substitute in the known values and solve for the unknown z's, or factor scores of individuals. The factor scores represent a person's status on traits which are in some sense fundamental, as opposed to the test scores, which represent more or less accidental combinations of these fun-

damental traits. One may think that vocational guidance can be accomplished more wisely and more efficiently on the basis of the fundamental factor scores than the original test scores. Thomson (1946, pp. 114-116) has proved, however, that no prediction can be made more accurately on the basis of factor scores than on the basis of the original test scores from which the factor scores are derived.

In order to understand this apparently paradoxical state of affairs, one should recall that each test will have only part of its variance accounted for by common factors; the remainder of the variance is said to be due to specific factors not shared with other tests in the battery and to errors of measurement. Nothing can be determined about specific and error factors in the course of factor analysis. They appear as a margin of error, a "plus or minus," which reduces the exactness, and therefore reduces the efficiency, of the derived factor scores. Looking at the matter a different way, there cannot be any information in the factor scores not already contained in the test scores.

A solution to this problem appears to be the construction of tests to measure the fundamental factors directly, and the use of these test scores in vocational guidance. That the construction of pure factor tests is one of the aims of factor analysis is stated repeatedly in the writings of Kelley, Thurstone, Cattell, Guilford, Thomson, and others.

Kelley has written, "It ordinarily happens that tests as drawn up are of the nature of the tests *A*, *B*, and *C*, each measuring more than a single mental function. The problem is then twofold: first, a determination, having tests *A*, *B*, and *C*, of what the independent mental traits are; and secondly, an experimental construction of new tests measuring these independent traits" (Kelley, 1928, p. 33). The latter problem, he admitted, was not solved in the course of his work.

Guilford (1947, p. 849) cites as one of the virtues of factor analysis that it "leads to the discovery and development of pure tests whose contributions are unique." Similarly, Cattell writes, "Psychometry needs guidance as to the nature of the primary factors. . . . As soon as these basic traits are made clear, the pro-

duction of . . . standardized tests can be depended upon to ensue quickly" (Cattell, 1946, p. 16).

The precise steps by which the results of factor analysis are utilized in improving tests have never been published, however. Thorndike (1948) has recently stated, "In spite of the devoted attention of Thurstone and other able workers, factorial analysis has not so far increased our equipment of adequate tests of pure abilities much if at all."

In order to present factor analysis in greater detail, we must turn now to the separate systems of methods proposed by various authors. The novice in this field may be baffled by the amount of contentious writing about factor analysis, especially as one of the contentions of certain proponents of factor analysis is that it provides a completely objective solution to the problems of mental organization. The reader will do well to remember the reply of the farmer who was asked whether he believed in baptism. "Believe in it? Why, I've seen it done!"

To find one's way, one must distinguish three types of propositions, those for which the evidence is a mathematical proof, those for which the evidence is empirical data, and those for which the evidence is intuitive or "rational." None of these types of propositions is exempt from the possibility of error. The most common errors in mathematical proofs relate to the underlying assumptions; the assumptions may not be completely stated, or they may be stated without consideration of their applicability in context, or they may be stated erroneously to be applicable in a context where, in fact, they are not applicable. Empirical data are subject to random and systematic sampling errors; moreover, we never interpret them directly, and the statistics which mediate our interpretation may be deceptive. The vagaries of intuition and "reason" are, of course, just the characteristics of the philosophical method that factor analysis was designed to obviate.

Unfortunately, the authors of original papers in the field of factor theory often have been far from clear about the nature of the evidence for the propositions they uphold. The task of comprehending and evaluating factor theory is complicated, in fact, by the frequent presentation of the assumptions in the same voice as the conclusions.

Methods of Factor Analysis.—*The Two-factor Method.*—Although factor analysis is usually thought of as a recent development, its origin can be traced to a paper by Spearman in 1904. At that time Spearman suggested, "All branches of intellectual activity have in common one fundamental function (or group of functions), whereas the remaining or specific elements of the activity seem in every case to be wholly different from that in all the others" (Spearman, 1904). The empirical basis for this suggestion consisted in certain observed regularities in a number of tables of correlation coefficients. The original statistical techniques were later abandoned, and the method is most profitably discussed in terms of later techniques.

The chief statistic of the two-factor method is known as a tetrad difference. Given four tests, which we may denote by w, x, y, z, then the tetrad difference is formed as follows:

$$r_{wx} r_{yz} - r_{wz} r_{xy}.$$

When this quantity equals zero, within the limits of its sampling error, then one says that "the tetrad equation is satisfied" for those four tests.

The "two-factor theorem" states that when the tetrad equation is satisfied for four tests, then and only then can the correlations between the tests be accounted for by a single factor common to all four tests plus a specific factor in each test. Spearman has labeled this common factor g, at least for those sets of tests which he deems to be intellectual in nature.

One could easily get the impression from Spearman's writings that he has proved that all mental tests are of the form:

$$X_{ij} = a_{gj} g_i + s_{ij}, \tag{2}$$

where X_{ij} is the score of individual i on test j, g_i is the value of g for individual i, a_{gj} is the loading of the g factor in test j, and s_{ij} is the value of the specific factor for that individual and that test, the part of the score that cannot be predicted by knowing g.

Certainly neither Spearman nor anyone else has proved all tests to be of the form of equation (2). To say that Spearman assumes all tests to be of that form would be closer to the truth. What the

two-factor theorem states is that for four tests which we can assume to be of the form given by equation (2), satisfaction of the tetrad equation is a necessary and sufficient condition to prove that the g factor is the same for all four tests, that is to say, the four tests involve one and only one common factor.

Empirically, how widely does the tetrad equation hold? Originally Spearman intimated that it would hold practically universally, but he quickly came to acknowledge that two tests as much alike, say, as canceling the *a*'s on a page of letters and canceling the *e*'s on a page of letters would have an element in common not shared with other tests. As evidence accumulated, he acknowledged the existence of general factors other than g and of group factors, i.e., factors common to some but not all of a battery of mental tests.

Spearman's g is somewhat similar to the general intelligence of the philosophic approach; it has the advantage that, if we accept the linear assumption, the assumption that factors add up to produce a score on a test, we can determine the weight of g in any test by the use of further formulas based on the tetrad equation. Then the best measure of g will be the test in which g has the greatest weight.

Spearman also emphasized that his two-factor theory provided a rational basis for the use of the Binet type of test as a measure of general intelligence. In such a hotchpotch of items, the effects of specific factors occurring in one item but not another tend to cancel each other out, while the general factor running through all items cumulates to determine largely the total score.

A much discussed "test" of the applicability of the two-factor theory was conducted by Brown and Stephenson (1933). They gave to 300 boys homogeneous as to age a group of 22 tests which, on the basis of a priori considerations and previous experience, were all of a cognitive nature and did not involve special overlaps. They manipulated their data in ways which other psychologists have criticized, even, for example, dropping one test from the battery because it produced large tetrad differences. They then came to the conclusion that the remaining tests satisfied the tetrad criterion within the limits of the appropriate sampling error.

The consensus of psychologists surely has been that if we use the tetrad difference equation to pick and choose among our tests, then

we cannot use the same data to prove that the human mind is so constituted that there is one and only one general ability running through all intellectual performances. In recent years there has been little use of the tetrad equation, and the "two-factor theory" has been succeeded by the bi-factor and other similar methods of factor analysis.

The Bi-factor Method. The bi-factor method of analysis has been developed largely by Holzinger and his students and co-workers, but it is much like the group factor method developed chiefly by Burt (1941). The basic assumption of the bi-factor method may be stated as follows.

$$(3) \qquad X_{ij} = a_{gj} g_i + a_{kj} z_{ik} + s_{ij} ,$$

where X_{ij} is the score of the ith person on the jth test, g_i is the score of the ith person in the general factor running through all the tests, a_{gj} is the weighting of the general factor in the jth test, z_{ik} is the score of the ith person in the kth factor, a_{kj} is the weighting of the kth factor in jth test, and s_{ij} includes the specific and error factors for the ith person on jth test. All scores except s_{ij} are in standard form. The subscript k runs from one to r, where r is some number very much smaller than m, the number of tests. All factors, general, group, and specific, are assumed to be uncorrelated with all other factors.

The factor denoted in equation (3) by g and the several factors denoted by the z's are called common factors, meaning they are common to more than one test in the battery, as opposed to the specific factors. The factors denoted by the z's are also called group factors, meaning that they run through some but not all the tests, as opposed to the general factor, common to all the tests in the battery. Although the distinction between common factors and the general factor may seem arbitrary and confusing, this usage is the accepted one and will be followed without further explanation.

Essentially, the bi-factor method sorts the tests into groups. For the tests in each group there is assumed to be one common factor, in addition to the general factor running through all tests of all groups. Holzinger has at times suggested the rejection of tests which do not have their variance sufficiently accounted for within

this pattern. Such complex tests, he suggested, are not good tests for factor appraisal (Holzinger and Swineford, 1937). At the same time he has suggested the modification of the bi-factor pattern so that a few tests will show loading in more than one group factor.

Sorting of the tests into groups, although it requires a good deal of computation, is not done on the basis of any rigorously deduced mathematical criterion. The coefficient of belongingness used for this purpose requires the exercise of considerable judgment. Two people starting with the same data might get somewhat different results for this reason.

The Method of Principal Components.—Two somewhat different methods of analyzing the variance of a battery of tests into principal components have been developed by Hotelling (1933) and Kelley (1935).

The first factor extracted by this method accounts for as much as possible of the score variance of all the tests The second factor accounts for the maximum possible part of the variance of all the tests when the effect of the first factor is mathematically removed. The third factor accounts for as much as possible of the variance remaining after the removal of effects of the first and second factors, and so on.

The factors are determined in such a way as to be uncorrelated. There are as many factors as there are tests, but the last few are likely to be of small importance. Specific factors are assumed to be nonexistent or of just the magnitude that would be accounted for by the unreliability of the various tests.

Although the method of principal components possesses greater mathematical elegance than other methods of factor analysis, it has not come into wide use. The mathematical elegance is purchased at the price of heavy computational labor, and apparently many psychologists believe that the factors so derived do not satisfy intuitive requirements for psychological meaningfulness.

The Multiple Factor Method.—The method of multiple factor analysis has been developed principally by Thurstone and his students. The term covers two distinct methods, the centroid method for extracting factors from the correlation table, and the "rotation"

of these factors to a configuration deemed to be more meaningful psychologically. The centroid method will not be considered here; about the same final results would be obtained if the correlation table were factored by the method of principal components (modified to exclude specific variance from the analysis), provided the factors found by this method were then rotated to conform to Thurstone's criteria.

In order to understand the meaning of rotating factors, one must master the geometric representation of factor analysis. Thomson (1946, Ch. 4) gives a lucid and relatively nonmathematical presentation of this topic. A crude notion of rotation may be obtained from the following illustration.

Suppose the streets of a town are laid out northwest to southeast and northeast to southwest. Jones, who is at the town school, wishes to visit Smith, who lives two miles due north of the school. Smith could tell Jones that he lives two miles north, but Jones will be better off with the knowledge that he should proceed 1.4 miles northwest and then 1.4 miles northeast. In this example, the school represents the zero point or origin of the descriptive axes. There are two dimensions, or axes. When the points of the compass are used, one axis is oriented north-south and the other east-west. The same map can be described in terms of axes which coincide with the town streets, in which case one axis runs northwest to southeast and the other runs northeast to southwest. The process of changing from describing the map by the compass axes to describing the map by the street axes corresponds to a rotation of axes. This change is accomplished by means of well-known equations.

The two-dimensional illustration will accommodate a problem with only two common factors. The axes in the illustration are at right angles to each other, in the language of factorists, the axes are orthogonal. Orthogonal axes correspond to the assumption that factors or abilities are independent, i.e., uncorrelated with each other.

In some cases, factorists prefer oblique or correlated axes. Conceivably the streets of our hypothetical town could be laid out so that the simplest way to go from the school to Smith's house is to proceed 2.8 miles northwest and then two miles due east. Such a layout of streets would be analogous to oblique factors or axes.

To change the analogy, let us suppose that we have a map of a little settlement of houses and a schoolhouse. No roads have been made. The townspeople desire to lay out two roads intersecting at the schoolhouse. The problem is to choose the directions for the roads.

The houses in this analogy correspond to the battery of tests. The "map" is drawn to correspond to the correlations between tests. Using the schoolhouse as origin or zero point, the correlation between two tests determines the angle made by lines drawn from the points representing those tests to the origin.

In practice, to represent the correlations accurately we need as many dimensions as we have tests. One of the assumptions of factor analysis, however, is that we can represent the essential features of the relations between tests by fewer dimensions than tests with only a small loss in accuracy.

One method of laying out the streets would be to choose the first or main street so as to be as close as possible to all the houses, and to choose the second street at right angles to main street, with the school at the intersection. The first factor of both the bi-factor method and the method of principal components is such a main street The second factors differ for the two methods, but this analogy is not rich enough to express the difference.

In the method of laying out streets analogous to the multiple factor method, we do not have a main street. Rather, the streets are laid out so that, as nearly as possible with a limited number of streets, each house is on some street. If there are two clumps of houses located with reference to the school so that streets at right angles to each other will pass through the center of each clump, then we have an "orthogonal solution." But if the streets can be made to come appreciably closer to the houses by orienting the streets at some angle other than a right angle, then we have an "oblique solution," which is just as satisfactory, at least to some factorists

A presentation of n-dimensional geometry cannot be attempted here. Our two dimensional analogy enables us to portray only two common factors, and therefore does not give an adequate picture of factor analysis. Let us return, then, to the basic assumption of factor analysis, formula (1). The task of factor analysis is to assign nu-

merical values to the coefficients, the a's, of this equation. As there are m tests and r factors, we may summarize the results of a factor analysis in a table of m times r a's, usually called a matrix of factor loadings.

TABLE 12.2 MATRIX OF FACTOR LOADINGS

		Factor				Σa^2
		1	2		r	
Test	1	a_{11}	a_{21}	. ....	a_{r1}	h_1^2
	2	a_{12}	a_{22}	.. .	a_{r2}	h_2^2
				. . . .		
	j	a_{1j}	a_{2j}		a_{rj}	h_j^2
	m	a_{1m}	a_{2m}		a_{rm}	h_m^2

The differences between the different factorial solutions can be described in terms of the characteristics of this matrix, table 12.2, as well as geometrically.

Note, first, that the specific factors are not represented in this table. Two factorists beginning with the same table of correlations (Table 12.1) will not necessarily agree even on the number of common factors (r) needed to describe the correlations; so they may arrive at matrices of factor loadings (Table 12.2) which differ even as to the number of columns. There is no universally accepted and unambiguous criterion for how many factors are significant, i.e., how many factors represent real mental abilities as opposed to errors of measurement and sampling errors. If we add the squares of all the a's for any one factor, we have an indication of the importance of that factor in determining the variance of the given test battery. There are several criteria for deciding when this sum is so small that the factor should not be included in the final description of the tests.

If we add the squares of the a's across the rows rather than down the columns of Table 12.2, we have a series of quantities characterizing the tests rather than the factors. These quantities are called the communalities of the tests and are generally designated by the

symbol h^2. The communality indicates the proportion of the variance of the test accounted for by the systematic or common factors of the study; the remainder of the variance is called the unique variance and is considered to be accounted for by the unreliability of the test and by systematic factors not shared with other tests in the study. Theoretically, for any test h^2 will be less than the reliability coefficient; in practice, contrary cases have been reported.

The importance of the communalities lies in the fact that by no method can the table of intercorrelations, Table 12.1, be submitted to factor analysis until quantities are entered in the diagonal cells, and Thurstone, Holzinger, and most other factorists agree that the communalities of the tests are the appropriate entries. Kelley and Hotelling prefer either unities or test reliabilities. Some texts of factor analysis have recommended the use of communalities in the diagonal cells when computing principal components, but Cureton (1939) believes the meaning of the components is thereby changed.

One of the major difficulties of factor analysis is that the communalities can be computed from Table 12.2, but Table 12.2 is found by analyzing Table 12.1. And we need to enter the communalities into Table 12.1 to analyze it. In order to get around this difficulty, there are various ways of estimating the communalities. The better ways are generally the more laborious.

According to the bi-factor method of analysis, each test in a battery of mental tests should have a positive loading on the first or general factor and a positive loading on just one other factor. Thus each row should have $r - 2$ zero or approximately zero entries. When the correlations between tests cannot be adequately accounted for within this pattern, however, Holzinger permits a few tests to have a total of three significant loadings, and significant negative loadings appear occasionally.

The method of principal components results in a table of factor loadings in which, in general, each factor is permitted to have a significant loading in each test. All loadings of the first factor will be positive, but for subsequent factors there will be positive and negative loadings.

The configuration of factor loadings which Thurstone deems an acceptable solution in the factorization of mental tests he calls "simple

structure." Table 12.2 is said to exhibit simple structure if it has the following characteristics. (a) Each row must have at least one zero. (b) Each column should have at least r zeros. (c) For every pair of columns there should be several tests (rows) with zero entries in one column but not in the other. (d) For every pair of columns there should be only a small number of tests with nonvanishing entries in both columns.

The first requirement insures that each test will be describable in terms of fewer factors than are involved in the whole battery. When this requirement is not satisfied by a few tests, Thurstone believes those tests should be discarded. The initial battery of tests should be large enough to permit the discarding of any such tests (Thurstone, 1947, p 335). How frequently Thurstone and his students discard tests on this ground is not clear, but there is no reason to think the practice is common.

The second and third requirements are necessary for the overdetermination and distinctness of the factors Distinctness means, of course, that two factors shall not coincide The notion of overdetermination is an important one. One point underdetermines a line, i e , there is an infinity of lines that can be drawn through any point. Two points just determine a line, i.e , there is one and just one straight line through any two distinct points. Three points all of which fall on the same line overdetermine that line. The requirements of simple structure are about equivalent to saying that, when we find three or more of the points representing tests falling on a single straight line, then we begin to look for a psychological meaning or ability underlying that regularity.

Note, however, that the requirement of a considerable number of vanishing entries in each column makes it impossible to discover a general ability (or "main street") as part of a simple structure. A general factor has appreciable positive loadings in all tests, a condition that cannot hold for any factor under the requirements of simple structure.

In addition to the above criteria of simple structure, when all the tests are concerned with abilities as opposed to attitudes and personality traits, a "positive manifold" is required. This requirement is that all entries in Table 12.2 be positive or zero. The reasons

for this requirement are the intuitive one that the notion of ability is essentially positive, and the empirical one that correlations between tests of abilities are almost invariably positive or zero.

The application of the criteria for simple structure is by no means a simple matter. For one thing, "zero" entries are not exactly zero, but zero within the limits of sampling error, and the sampling errors cannot be ascertained exactly.

Thurstone states that where there are r factors, one must draw $r(r-1)/2$ diagrams to judge whether the requirements for simple structure are satisfied. When all of these diagrams have the appropriate characteristics, "we say that the structure is 'compelling,' and we have good assurance that the simple structure is unique. In the last analysis it is the appearance of the diagrams that determines, more than any other criterion, which of the hyperplanes of the simple structure are convincing and whether the whole configuration is to be accepted as stable and ready for interpretation" (Thurstone, 1947, p. 335).

In addition to the formal requirements of a positive manifold and simple structure, the acceptability of factors discovered in any study depends upon "the plausibility of the theoretical interpretation that the investigator may be able to find" (Thurstone, 1947, p. 337). The plausibility of the interpretation is to be considered for each factor in the configuration separately.

Finally, experimental tests of hypotheses suggested by factor analysis are used to validate results. Thurstone cites as an example the verification of the difference between the verbal factor and the word fluency factor. Tests constructed to measure one rather than the other behaved as predicted in subsequent factor analyses (Thurstone, 1947, p. 338).

In recent years the multiple factor school has put increasing emphasis on oblique as opposed to orthogonal factor structure. When the factors are oblique, we can draw up a table of intercorrelations between factors which will look just like a table of intercorrelations between tests. The possibility then exists of performing a factor analysis of the factors. The results of this analysis are referred to as second order factors.

Many psychologists have felt that second-order factors contain the possibility of reconciliation of the followers of Thurstone and those who believe in a general factor in cognitive tests, because a general factor may appear in the second order analysis when none has appeared in the original analysis. The appearance of this general factor may, however, be a methodological artifact.

The number of factors that are determined by any analysis is limited by the number of variables being analyzed. The number of factors is always considerably less than the number of variables. Usually one does not bother to conduct a factor analysis unless there is a fairly large number of tests. Most factor analyses end up with five or ten factors or perhaps even fewer than five factors. When these factors are then subjected to a second order analysis, there cannot possibly result more than one or two or three second order factors The likelihood of finding a single common factor among the factors is thus much greater than the likelihood of finding a general factor among the original tests, for purely methodological reasons.

Thomson says of second-order factor analysis, "Whether such a procedure could be justified by the reliability of the original experimental data is very doubtful in most psychological experiments. The superstructure of theory and calculation raised upon those data is already, many would urge, perhaps rather top-heavy, and to add a second storey unwise" (Thomson, 1946, p. 293).

One should remember that the second-order factor analysis makes assumptions corresponding to those made by the original analysis. The first-order factor scores are assumed to be linear functions of the second-order factor scores, just as the test scores were previously assumed to be linear functions of the first-order factor scores (equation 1). But if these assumptions hold simultaneously, then the test scores must be linear functions of the second-order factors. How does it happen that the first-order factors and not the second-order factors turned up in the first analysis? What determines which shall emerge as first-order and which as second-order factors? Some answer must be made to this question if second order factors are to be invested with psychological sense.

Choosing among Factorial Methods.—Is there a crucial basis for choosing among the various methods of factor analysis? In the

United States, most psychologists have favored the Thurstone school of multiple factor analysis, but in Great Britain methods yielding a prominent general factor are preferred. Is the choice a matter of convenience, of purpose, of fashion?

Wolfle (1940) and Holzinger and Harman (1941) have discussed criteria for choosing among factorial systems. Many of their criteria will not be included here, for they describe differences between factorial systems but do not truly afford a basis for choice.

The ease with which the computation of a factor analysis of a given set of data can be completed is mentioned by Wolfle. According to Wolfle, Tryon's (1939) method of cluster analysis is the easiest to apply, Holzinger's bi-factor method is next in difficulty, while the multiple factor and principal components methods are hardest. In general, the exactness of the results of a factor analysis depends on the amount of computational labor one is willing to invest. All widely used methods involve approximations at various stages. Sometimes these approximations are used because exact values are unattainable, as in the estimation of test communalities Often the approximations are used because they are computational short-cuts.

Cluster analysis yields a kind of thumb-nail approximation to the results of multiple factor analysis, the goodness of the approximation depending to a considerable extent on the simplicity of the test configuration. For a more exact picture of the test configuration than cluster analysis affords, Tryon has proposed a method called orthometric analysis, which is, of course, more laborious than cluster analysis, although less so than Thurstone's methods. Cluster and orthometric analyses have not been described above because they have not come into wide use, and because they are essentially computational devices, and computational details have not been given for any of the methods. Wolfle recommends cluster analysis for situations in which a quick approximation to multiple factors is desired.

The method of factor analysis which is most satisfactory from the point of view of mathematical elegance is the method of principal components. Probably this is the only method with which two people, starting from the same data and making no computational errors, would arrive at exactly the same table of factor loadings. When

the total variance rather than the common variance is analyzed, i.e., when unities rather than communalities are entered in the diagonal of the table of correlations, the sampling errors of the factor loadings can be estimated more exactly than with any other method. This method also has not been widely adopted

As Thurstone's multiple factor analysis has been widely used, apparently psychologists have felt that the points of superiority claimed for his method are the crucial ones His chief claims are that factors discovered by his method lend themselves more readily to psychological interpretation than those discovered by other methods, and that these factors are in some sense invariant Whether these are two separate virtues or two aspects of the same virtue, he does not make explicit, nor does he explain the nature of the evidence for these propositions

With regard to the ease of the interpretation of the factors, Wolfle states, "Thurstone's primary factors, Spearman-Holzinger group factors, and Tryon's operational unities may be fairly readily interpreted The interpretations given to Tryon's factors will frequently be more complex than those of the Thurstone or Spearman-Holzinger group factors The general factor of the latter method presents peculiar problems The factors of the Hotelling-Kelley method present considerably greater difficulty than any of the others" (Wolfle, 1940, p 21)

Certainly Wolfle's opinions, based on a complete survey of factor analysis literature to 1940, are worthy of consideration; however, these impressions would be more valuable to us if he explained whether their basis was intuitive, mathematical, or empirical Earlier in his review he gave an example of a particularly complicated interpretation of a factor obtained by the method of principal components But one example is hardly crucial Thurstone does not guarantee to interpret every factor, and there are many factors in the literature of multiple factor analysis for which no interpretation is offered.

Wolfle does cite experimental evidence for Thurstone's claim that the factorial composition of a test does not depend on the battery in which it is analyzed, provided the factors satisfy the criteria

of simple structure. Most of the evidence is incidental to other studies involving some changes in population or tests or both. Since factor loadings are not expected to be identical under such changes, and since the identity of the factors from one study to another is often open to question, the evidence cannot be evaluated strictly, nor does Wolfle attempt to do so. On intuitive grounds there does seem reason to believe, with Wolfle, that the primary traits of multiple factor analysis may yield more stable pictures of tests and people than Holzinger's bi-factors or Tryon's clusters and operational unities. Certainly principal components, unrotated, are bound to change with changes in the test battery.

Wilson and Worcester (1939) have demonstrated that the inclusion of a test twice rather than once can produce appreciable changes in first factor loadings by the method of principal components. Obviously, the double inclusion of a test does not change any underlying traits. Kelley (1940) has answered that the selection and weighting of the initial tests are the crux of the factorial problem. In an earlier work Kelley (1935) described a method for performing this initial step of selecting and weighting measures by pooling judgments of experts. Probably many psychologists feel that Kelley's method deprives factor analysis of its claim to being an objective solution to the problem of mental organization.

The invariance of factor loadings of a test is an important criterion of the value of factor analysis in general and of one factorial method as against another. Better experimental tests of this property could be set up, giving due regard to the fact that the factorial composition of a test will normally be affected by age changes in the population, by differing training on similar tasks, by different methods of solving a test, and by changes in a test battery such that what is specific variance in one battery becomes common variance in another.

Although multiple factor analysis has to its credit many studies yielding intelligible and more or less consistent pictures of the human mind, there are still outstanding objections to it. The criteria for simple structure are such that no factor can have an appreciable loading in all tests. In effect, Thurstone has decided in advance that his idea of the most parsimonious picture of the human mind does

not include a general factor. Those psychologists who find the notion of general intelligence a congenial one will be dissatisfied with simple structure as a criterion for primary mental abilities. Nor is simple structure without rivals as a criterion of the most suitable configuration toward which to rotate factors.

Reyburn and Taylor (1943) have challenged, on empirical and theoretical grounds, Thurstone's claim that factors exhibiting simple structure will result necessarily in invariant factor loadings for tests and invariant factor scores for people. They propose a method of rotation which puts greater emphasis on the psychological reasonableness of factors and factor loadings

Cattell (1944) has examined a number of alternative principles for the rotation of factors. He favors the principle which he calls "parallel proportional profiles" as an alternative to simple structure. The most apparent of the difficulties in applying Cattell's principle is the amount of work involved, for the method involves the simultaneous rotation of factors from two or more analyses.

The bases for choice among factorial methods which have just been enumerated are all intrinsic to factor analysis. Yet one can conceive of a system of factor analysis which is efficient, mathematically elegant, self-consistent from experiment to experiment, and entirely solipsistic in ultimate outcome. The last word on the usefulness of factor analysis, one method against another or factor analysis as a whole, must come from outside factor analysis. If factor analyses enable us to predict nothing but future factor analyses, then the world will be no poorer for forgetting all about it. If factor analysis facilitates some predictions extrinsic to itself, then we have a right to know what predictions and to ask how much these predictions are improved by knowledge derived from factor analysis.

The amount of evidence concerning the ability of factors differently derived to predict outside criteria is meager. Wenger, Holzinger, and Harman (1948) analyzed the intercorrelations of a single battery of tests given to school children in the age range 10 years 6 months to 13 years 6 months by means of the bi-factor and multiple factor techniques. For each pupil estimates were then made of scores on the factors derived by both methods. The general factor obtained by the bi-factor method showed consistent and moderately

high correlations with school marks throughout the age range. The group factors of the bi-factor method showed negligible correlation with school subjects. The verbal factor of the oblique multiple factor solution showed correlations with school subjects about equal to those of the general factor. The other three factors of the oblique solution had smaller correlations with school marks, but they were consistent and appreciable. According to the authors, the general factor of the bi-factor analysis has a smaller error of estimate than the oblique factors. They therefore believe that it is an open question whether school grades can be predicted better by using all the factors of a primary oblique solution or by using the general factor of a bi-factor solution. The latter is, they assert, more practical. There should be many studies on the order of this one before a decision is reached on the method of factor analysis yielding the most valid and useful picture of the human mind.

Results of Factor Analysis.--Wolfle lists as the six cognitive factors most frequently identified, in approximate order of frequency, "verbal ability, ability to deal with spatial relations, simple numerical ability, memory (probably immediate memory), mental or perceptual speed, and ability to grasp logical rules and relations" (Wolfle, 1940, p. 33).

The verbal factor has been identified by means of every factorial method. "It shows up most clearly in reading tests, verbal analogies, synonyms, opposites, vocabulary, grammar, verbal associations, and similar tests. It is involved primarily in those tests which depend upon the meanings of words and the ideas associated with them" (Wolfle, 1940, p. 31).

The space factor has also been identified by all methods of analysis. "It appears prominently in tests requiring the subject to react to spatial relations, to read plans or blueprints, or to tell quickly whether two drawings represent the same or opposite sides of such asymmetrical figures as flags" (Wolfle, 1940, p. 31). More recently, Wenger, Holzinger and Harman (1948) have suggested that the space factor might more properly be identified as a visual imagery factor.

The number factor has been found by all methods except possibly that of principal components. Some well-known studies, such as Kelley's *Crossroads in the Mind of Man* (1928), were important in the development of factorial methods but did not use a method exactly like any of the four major ones described in the preceding sections. One should remember also that the fact that a given factor has not been identified by a certain method of analysis may be purely accidental, reflecting only that the type of test in which the factor occurs has not been present in analyses using that method. Wolfle says, "The number factor appears most clearly in the simple numerical operations of multiplication, addition, subtraction and division. It is less heavily weighted in more complex tasks, such as numerical reasoning" (Wolfle, 1940, p. 31).

There is some question as to the nature of the memory factor, which has been identified in studies using tetrad differences, the bi-factor method, and the multiple factor method. Wolfle suggests that it would be better to call it a rote learning or immediate memory factor, but points out that tests of memory over longer intervals were not included in the test batteries. Some studies have raised the question as to whether there are several memory factors.

The speed factor has been even less well identified. It has appeared in several studies, using the bi-factor and other methods. DuBois (1932) suggests that it appears in mental tests whose content is easy for the subjects tested. Thurstone and others have found a factor which they call perceptual speed, which may or may not be the same factor. Thurstone has described the factor in terms of the discovery and identification of perceptual detail. The differences of opinion about the nature of the speed factor are more likely to be a function of test batteries and subjects than of the method of analysis used. Cattell, in a summary more recent than that of Wolfle, has said, "How many restricted group factors are needed to account for speed performances must remain, with present research data, speculative" (Cattell, 1946, p. 423).

With regard to the last factor or series of factors in Wolfle's list, the differences of opinion arise in large part from the method of analysis. Thurstone (1938) at one time identified separate induction and deduction factors. Later studies failed to confirm the identifica-

tion of the deduction factor. He has subsequently more or less withdrawn the deduction factor from his list of primary traits and renamed the induction factor "Reasoning." The reasoning factor showed a high correlation with a second-order general factor in a study of eighth grade children (Thurstone and Thurstone, 1941). Guilford (1947, p. 122) found three reasoning factors, but they were not identifiable as inductive or deductive.

Holzinger and Swineford (1939) included reasoning tests of the induction and deduction types in bi-factor analyses Instead of finding group factors in these tests, they found the tests to have high loadings in the general factor of their method. These results indicate that the general factor of cognitive tests may be primarily a reasoning factor. Spearman (1927) had long previously suggested that his *g* was the ability to find relations and educe correlates.

Wolfle's list of factors still covers fairly well the categories in which factors can be classed, but each of his six factors, with the possible exception of the number factor, has been broken down into several clearly distinguishable and more or less independent factors in subsequent studies.

The number factor has been shown to extend over tests involving incidental use of numbers without any computation (Guilford, 1947, p. 118; Swineford and Holzinger, 1942, p 20; Wright, 1939). This result suggests that an emotional blockage against numbers may explain or at least contribute to this factor, so that the single best established factor may have an emotional complex rather than an ability as its psychological basis!

To bring Wolfle's list of "established factors" up to date does not seem at present a profitable enterprise A complete list of factors that have been found, even if we confined attention to studies employing the criterion of simple structure, would be so long as to be of no help in thinking about the human mind. Guilford (1947) lists 28 as having been found in the Army Air Forces studies alone. If we demand further that the factors be found in more than one study, we encounter new problems. What guarantee do we have that two factors called by the same name are the same factor? Does the discovery of the same factor in two different analyses really imply independent confirmation of the factor? Let us begin with the first question.

Wolfle mentioned a special difficulty involved in interpreting a general factor of the bi-factor method. This difficulty is that the general factor in one test battery will not be exactly the same as the general factor in another test battery. While the difficulty certainly applies to the general factor, one cannot be certain it does not apply to other factors too. The fact that two psychologists both find a "verbal factor" in their respective test batteries does not guarantee that it is the same factor, for Thurstone (1948) has stated, "At least three verbal factors are known and several additional verbal factors are clearly indicated." Memory, space, and perceptual abilities have been similarly broken down into several separate factors. But even if a factor is further identified as referring to meaningful verbal materials and appearing in some of the same tests in two batteries, we cannot be sure of the identity of the two factors

The statement that two factors can be quite similar but not identical must be confusing to those who think of factors as permanent entities waiting to be discovered in a Platonic "world of forms." Burt (1941, p. 232), for one, has confessed that he is driven to such a Platonic view "by the immediate exigencies of factorial work."

Let us look at the real situation. We begin by giving a battery of tests to some convenient group. For reasons which are obvious to anyone who has faced the task of finding subjects for a lengthy experiment, most factor analyses of human ability have been performed on students from grade school through college. While the grade school students form fairly representative samples of their communities, the college students are undoubtedly highly selected from their age groups. The Army Air Forces studies were done on groups for which the selective standards varied but were probably never less than for college freshmen. Whether such selective factors relate to the organization as well as to the amount of abilities cannot be judged at present.

Concerning errors due to random sampling, McNemar (1941) found experimentally that under conditions similar to those in some of the more painstaking factorial studies, large loadings may occur by chance for factors beyond the first one. He states further:

"In other sampling situations it is usually easy to conceive of a universe value about which sample values will disperse them-

selves. In the factorial situation, it is difficult to see just what the universe value or values might be. The statistic which varies from sample to sample is the factor loading or projection on a centroid axis which itself varies from sample to sample. A mere rotation of axes, regardless of how clearcut the structure, will not change the predicament—the location of the rotated axes is subject to the chance vagaries of sampling, and of course, the test projections thereupon are also affected by sampling. In other words, we have no stable reference frame from which to regard the sampling fluctuations of test projections" (pp. 147-48).

What of the tests? There have never been unambiguous rules set down for the kinds of test scores to which factor analysis can be applied legitimately. Such rules as have been stated refer at least in part to properties of the tests which cannot be ascertained readily. For example, Thurstone (1937) has stated rules concerning the factorial composition of tests suitable for factor analysis, but the only means of ascertaining the factorial composition prior to performing a factor analysis is that old stand-by of the philosophic method, intuition.

What Thurstone sometimes calls the "score function" of a test is not a mathematical function at all, but simply the sum of the scores on a number of separate items which may measure a variety of abilities. There are no generally accepted rules for constructing tests of ability (Loevinger, 1947). Recently homogeneity of test content has been suggested as one criterion for the suitability of a test for factor analysis (Loevinger, 1948), but homogeneous tests of ability have not yet been constructed.

Most of the tests in Thurstone's (1938) battery of fifty-seven, used for his best known study, appear prima facie to be highly homogeneous. Some psychologists have criticized the tests, however, for achieving this degree of homogeneity by being primarily speed tests and have raised the question whether the same factors would appear in a similar battery of power tests.

In addition to the problem of homogeneity of test content, Thurstone has recognized that the unit of measurement of the tests, which presents unsolved problems, may influence the results, and that the basic linear assumption, common to all methods of factor analysis in one modification or another, is open to question. Furthermore,

with the possible exception of the method of principal components, all methods of factor analysis require a number of approximations and the exercise of considerable judgment, so that two analysts starting with the same data would not be likely to arrive at exactly identical results. There is insufficient evidence on the question of whether the results would differ to an extent of practical importance.

When we keep in mind all the difficulties, weaknesses, and approximations of the factorial methods, we are prepared to understand that the "verbal" or "spatial" factor in one analysis may be neither wholly different from nor wholly the same as the "verbal" or "spatial" factor in another analysis.

The first seven factors of Thurstone's (1938) initial study have reappeared in many subsequent studies. Often these studies have been planned to include tests the same as or similar to those most heavily weighted with these factors, giving the maximum possible assurance of the similarity of the new and the old factors. The very facts which insure the similarity of the factors, however, make the reappearance of the factors difficult to evaluate. Since the criteria for simple structure are sufficiently subjective to permit the investigator to be influenced by knowledge of the results of previous studies, the reappearance of these factors cannot be considered independent confirmation of the original discovery. On the other hand, in some instances factors which were intentionally introduced into the study did not turn up in the final factorial structure. Coombs (1941) recovered a number of factors he put into his study even though the rotation was performed "blind," i.e., the tests were known to the person doing the rotation only by code number rather than by name or by content. Blind analysis, however, is neither widely practiced nor widely advocated.

Interpretation of Factors.—Suppose we grant that the mental test scores of high school and college students are expressible as the weighted sum of half a dozen or more factor scores. What do the factor scores represent? What is a factor?

Spearman, whose *g* was the first factor to be proposed, also supplied the first interpretation of a factor. "The facts of general psy-

chology . . . strongly support the suggestion of mental energy and engines. Moreover, such an energy would seem to be just what is wanted to explain *g*, whilst the engines might go far towards explaining the *s*'s" (Spearman, 1927, p. 135).

Kelley (1928) entered a number of technical objections to Spearman's finding of a universal general factor and offered a different interpretation of the acknowledged tendency toward positive correlation among tests of ability. Among the causes of this positive correlation Kelley lists heterogeneity of subjects with regard to maturity, racial origin, nurture, and sex. Other possible causes are selection of tests very similar to each other and an intrinsic similarity in all mental activities. In so far as the positive correlations among mental tests are caused by selection of tests and of subjects, they are not caused by similarities in mental functions. Kelley acknowledged a difficulty in deciding just how far one should carry homogeneity in selection of subjects, but he felt that the data then available could be accounted for without postulating a central intellective factor other than heterogeneity with respect to age, sex, race, and training.

The issue between Spearman and Kelley has not been resolved but merely shifted to new ground. Kelley was using a modification of the tetrad difference technique, which is no longer in use. Whether a factorist will find a central intellective factor today depends in part on his choice of techniques; each factorial method is prejudiced either in favor of or against a general factor. Kelley (1935), in developing the method of principal components, has subsequently committed himself to a general factor.

Holzinger and Harman (1938) analyzed by the bi-factor method the data of Thurstone's initial study. While Thurstone found no general factor, they found an appreciable one. "The presence of this factor in our pattern is due to our hypothesis of its existence and the essentially positive correlations throughout," they commented.

Guilford (1941) expresses the belief that a general factor will not necessarily escape detection by Thurstone's method, provided the criteria of a positive table of factor loadings and a maximal number of zero loadings are substituted for the stricter criteria of simple structure. Factor analyses by Goodman (1943), Wright (1939),

and Blakey (1940), all using Thurstone's methods, revealed general factors. The studies of Wright and Blakey were done under Thurstone's direction.

Kelley (1928, p. 22) remarked, in this connection, that if there is a battery of tests in which *A* is a general factor, *B* a group factor, and *C* a factor specific to just one test, there could be another battery of tests in which the *A* factor would be a specific, the *B* factor general, and *C* a group factor.

For one important battery of tests, the 1937 Revision of the Stanford-Binet scale, McNemar (1942) has found that at every age level there is a general factor running through all items. Evidence indicates that the general factors of different age levels are almost identical. At a few age levels there is evidence for significant group factors, but the nature of the group factors is not entirely clear. McNemar points out, however, that these results characterize the Stanford-Binet scale rather than the human mind.

As matters now stand, the existence and nature of a factor of general intelligence is a moot question. Whether one discovers a general factor depends at least in part on the method of factor analysis employed and on the selection of tests. Having found a general factor, one may interpret it as general intelligence or as a factor due to the heterogeneity of the subjects, chiefly with respect to age. On the other side of this coin, the absence of a general factor may be due to homogeneity of the subjects with respect to general ability, a criticism which could be entered against Thurstone's original study.

The problem of interpreting group factors is still a subject of lively controversy. Anastasi (1937) and Tryon (1939), two of the strongest critics of factor analysis, have rejected the idea that factor analysis can reveal any psychological realities. Tryon protects himself from the accusation of reifying factors by substitution of the term "operational unities." In the same spirit, Anastasi (1937, p. 318) says a factor can not "be interpreted in terms of underlying psychological entities"; it is "simply a mathematical statement of observed relationships among a group of concrete behavior manifestations."

Thurstone has answered, "Some students of factor analysis deny that meaningful factors exist in psychological measurements or performances, at least when they are revealed factorially. In other

contexts the same authors speak confidently about 'auditory acuity,' 'mechanical aptitude,' 'fluency,' 'visualizing,' and many other components of abilities and disabilities; but, when these same functions appear in a factor analysis, they promptly discard the psychological interpretations as foolish" (Thurstone, 1947, p. 177).

Thurstone has stated frequently that some factors appearing even in a configuration showing simple structure may not yield to psychological interpretation. When, however, similar factors appear in similar tests in a variety of studies, it becomes imperative to account for the similarities. Wolfle (1940, p. 26) agrees with Thurstone that the reappearance of similar factors in different studies shows that factors are not artifacts.

At times Thurstone uses more elegant language to describe the nature of factors. The chapter on simple structure in his *Multiple Factor Analysis* (1947) opens with a discussion of the "score function." Each test score, he states, is some mathematical function of a set of "parameters." The parameters are the factors, and in order to perform the factor analysis, one must assume that the mathematical function can be reasonably approximated by a linear equation

In discussions of this nature, Thurstone is vulnerable on two points. First, test scores cannot be considered precise mathematical functions of anything, for as psychological tests are now constructed, two people with the same score will in general have done different items correctly. Thus the items must measure various abilities, and a given total score will represent different sets of abilities for different people. Secondly, the particular mathematical function which factor analysis can deal with, the linear one, is by no means always a reasonable approximation to the exact function, as factor analysts are prone to assume.

Are factors merely mathematical artifacts? What we mean by an artifact is an effect of the method of studying rather than of the thing studied. While the reappearance of factors may indicate that they are not artifacts of those aspects of method which change from one study to another, it does not indicate that the factors are not artifacts of those aspects of method which are always present. The fundamental assumption of factor analysis, equation (1), is one

aspect of the method which is basic to every factor analysis. What justification is there for this basic assumption? The closest anyone has come to justifying the assumption that test scores are a linear function of factor scores is to make brief reference to Taylor's theorem. That Taylor's theorem provides no such justification has been shown in detail in a slightly different context (Loevinger, 1943).

Jeffress (1948) has raised a more fundamental objection. He asks, not whether a given test always taps the same factors, but whether, even in a relatively homogeneous group, people can be assumed to have the same factors. Why should all minds be organized in the same fashion? May people differ not only in amounts, not only in mental profiles, but in the whole pattern of mental organization?

The question Jeffress raises is not entirely new. Stephenson (1936, Burt and Stephenson, 1939) has been making a similar point for some years. Stephenson's solution is to reverse the roles of tests and people in performing a factor analysis. Instead of analyzing a matrix of intercorrelations of tests, he proposed that we analyze the intercorrelations of people. In this way types of people will be isolated rather than factors of mind. At times Stephenson appears almost to believe that his "inverted factor analysis" makes no assumptions, in contrast to the usual method of factor analysis. Since he proposes no change in factor analysis other than exchanging tests with people, plus devices to make the correlation of people feasible, his method must make at least as many assumptions as the usual factor analyses. Methodological difficulties in correlating people make the assumptions of inverted factor analysis more difficult to formulate in psychological terms than those of conventional factor analysis. One may doubt, however, whether the statement that each person's test score is a weighted sum of his scores on fundamental types makes more sense than to say that his test score is a weighted sum of his scores on fundamental abilities.

Let us return for a moment to the question of whether factors are real. Has Thurstone defended this point sufficiently in showing that similar factors appear in different analyses? Bridgman (1927) has suggested as a definition of the physical reality of things not given

directly by experience that there should be operations to which they are uniquely correlated independent of the operations which define the objects.

> "The notion of 'physical reality' is not of prime importance to this discussion of the character of our constructs; our definition of the meaning of physical reality may not appeal to everyone. The essential point is that our constructs fall into two classes: those to which no physical operations correspond other than those which enter the definition of the construct, and those which admit of other operations, or which could be defined in several alternative ways in terms of physically distinct operations" (Bridgman, 1927, pp. 59-60).

Since there have been only hints of confirmation of factors by methods completely independent of factor analysis, the physical reality of factors, by this criterion, remains to be established. According to this view the question of the reality of factors is not altogether different from the question of the practical applications of factor analysis. Although the proponents of factor analysis promise to put vocational guidance on a scientific basis, and sometimes even to improve the school curriculum and perhaps the social order, there are no widely accepted practical applications of factor analysis.

One final interpretation of factors must be presented, for it has a large number of advocates. This interpretation is known as the sampling theory of ability and is associated chiefly with Thomson, although Tryon, Thorndike, Wolfle, and others have espoused similar views. According to this view every ability should be thought of as constituted by a large number of smaller bits, which may be identified with genes, neural arcs, segments of experience, or just left as small bits of ability. Every test samples a large number of these elements. The correlations between tests depend on the number of overlapping elements. The appearance of factors may be accounted for by chance or by assuming that these elementary determiners of ability are organized into more or less overlapping "sub-pools of the mind."

The sampling theory hardly qualifies as a true theory, for it does not make any assertion to which evidence is relevant. Perhaps the

large number of adherents to this view is due to the fact that no one has offered evidence against it. But until the view is defined more sharply, one cannot even conceive of the possibility of contrary evidence, nor, for that matter, confirmatory evidence. A statement about the human mind which can be neither supported nor refuted by any facts, known or conceivable, is certainly useless. Bridgman and other philosophers of science would probably declare the sampling theory to be meaningless.

An Operational Solution

What is intelligence? What is the nature of mental organization? The solution of these questions by arbitrary definition has not been considered satisfactory by most psychologists The pragmatists reversed the philosophic procedure. Instead of defining intelligence and then attempting to measure it, they constructed tests and said, "Intelligence is what this test measures." But the pragmatic solution also is considered arbitrary and not entirely satisfactory.

By far the most widely accepted solution to the problem today is that of the factorists. The factorists say, first we will construct an objective map of mental organization by analyzing the correlations of such tests as we have; then we will construct pure tests of the abilities objectively defined in our factor analyses. The program of the factorists may one day succeed in greatly enriching the science of psychology. At present, the first step in the factorists' program, the mapping out of the domain of intelligence, is subject to a great many difficulties, and the second step, the construction of pure tests of abilities, remains in the future.

From the operationalist view, as interpreted by the present writer, there are two major objections to factor analysis. An operationalist cannot subscribe to the factorists' program of first defining, then measuring. Rather this view states that the operations for measuring an ability constitute the definition of that ability. What is meant by "mapping out the domain," apart from constructing pure tests, is not at all clear operationally.

The second objection to factor analysis is that the fundamental additive assumption is unintelligible from an operational view. Let

us look at the matter this way. Some students are sitting in a room taking a mental test. The experimenter scores their tests, assigning plus or minus to each student on each item, and adds the item scores to form total scores. Or perhaps the total scores are time or error scores. Now the factorist says these total scores are the sum of a set of factor scores. Quite apart from the question of whether the addition of factor scores provides a reasonable approximation to the total, the operationist wants to know where the factors sit. At what point in the testing process do the factors combine to make a total score, whether by addition, multiplication, or some transcendental function? Where does this mathematical combination take place?

The technic of homogeneous tests (Loevinger, 1947, 1948) has been proposed as a method for the objective definition of abilities. In contrast with factor analysis, this technic deals solely with patterns of answers to items, rather than with the hypothetical addition of hypothetical mental factors. Tests which satisfy the criterion of homogeneity of content must measure something, though whether they measure essential aspects of the human mind or merely reflect accidental features of the test situation must be determined by other methods.

The view that homogeneous tests are essential to the investigation of the nature of intelligence has antecedents at least as far back as Ruml's (1921) contribution to the 1921 symposium on the nature and measurement of intelligence in the *Journal of Educational Psychology*. Most factor analyses have utilized tests which appear to be homogeneous The omnibus type of test is generally agreed to be unsuitable for factor analysis The superficial similarity of items is not, however, a guarantee that the same ability or the same complex of abilities will be measured by the various items The most illuminating tests will surely be those where the statistical criteria of homogeneity are satisfied despite the apparent dissimilarities in the content of the items.

The technic of homogeneous tests does not aim at solving exactly the same problems as factor analysis, since the first step in the factorial program, the definition of abilities apart from their measurement, is operationally impossible. Both aim at the construction of

"pure" tests of abilities. The steps involved in translating the results of a factor analysis into pure tests of the factors have never been published, nor have there been notably successful tests of pure factors. The steps in the construction of homogeneous tests are being defined, although empirical results of the use of these methods are not yet available.

Methods of test construction and analysis similar to the technic of homogeneous tests are being worked out by Guttman (1944), Coombs (1948), and others, but none of the other methods has been explicitly related to the problem of the objective definition of abilities.

SUMMARY

In tracing the nature of intelligence and of mental organization, we have taken a long methodological excursion with side-trips into philosophy of science But have we learned anything about the human mind?

The views of intelligence proposed in the period of the philosophic approach are still current in altered form. The argument between those who believe in general intelligence as opposed to those who believe in several faculties has shifted to a choice between factorial methods which lead to a general factor and those which exclude a general factor. One change has been that while faculties were most often thought of as referring to abstract, formal qualities of thinking, well-defined factors more often refer to the concrete content of test materials and mental operations. The sampling view of intelligence continues as an interpretation of both general and group factors.

While certain factors have been found repeatedly by use of a variety of factorial methods and a variety of groups, most of these factors have been found in other studies to split into smaller, more specific factors. The fact that a great many studies have found a verbal factor is made less impressive by recent studies which indicate that there are three or more different verbal factors. There has never been a clear outline of the conditions under which one study can be said to have provided independent confirmation of factors found in another study. We must therefore be reluctant to subscribe to any list of "known" factors.

The pragmatic view that intelligence can be measured perfectly well in advance of its definition probably still persists in its own right. Clearly it is akin to the notion that an ability for which a homogeneous test can be constructed is thereby defined. Thorndike's dictum that what exists can be measured has been reversed to say that what can be measured must exist.

Surprisingly enough, psychologists who specialize in other fields often think that the field of intelligence and mental measurement consists of a mass of facts, clearly defined and well accepted by all concerned. There are indeed a great number of empirical studies in the field, most of which have not been mentioned in this chapter Many of them are ambiguous as to whether they are exploring the properties of the human mind, of psychological tests, or of the particular statistical methods employed. Some of these studies must contain valuable insights into the human mind, but until improvements in methodology are made, one cannot easily separate the valuable from the misleading insights.

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CHAPTER 13

PERSONALITY

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All too often the chapter on personality in treatises of psychology is sadly out of place, disjointed from the rest of the text, appearing sometimes at the beginning but more frequently at the end of the book in isolation anything but splendid More often than one would wish, the chapter on personality makes little use of those concepts and principles that are employed in other chapters for the exposition and explanation of the major body of psychological data And the neglect goes the other way too; the time-honored topics of psychology are presented in the traditional manner unenlightened and unmodified by the findings of the extensive investigations of personality which have been made during the last half century in the clinical field and during the last quarter of a century in the experimental laboratory, and which now make mandatory a revision of general psychological theory.

There has been no dearth of textbooks of general psychology, but for the most part they have either slighted the topic of personality or, if they have attempted to offer a theoretical treatment of the problem, they have done so in a highly disjunctive manner.

The single exception to this statement has been William Stern's *General Psychology from the Personalistic Standpoint* (1938). This most remarkable textbook of psychology, written originally in German in a tradition of thought foreign to that of American psychologists, has been almost completely neglected in this country. As one reviews, as we shall in this chapter, the increasing rapprochement of general psychology and what, in the past, has been the special psychology of personality, one may venture the prediction that much that is to be found in Stern's psychology will be independently rediscovered and introduced into the general body of psychology, though,

to be sure, with a degree of quantification not found in Stern's treatment.

It has been Stern's great contribution to demonstrate that the traditional definitions of psychology are to be taken seriously, as indeed they have not been, even by their authors. It will be recalled that Wundt wrote of psychology that "It investigates the total content of experience in its relations to the subject" (Allport, 1937, p. 549); that James proposed that "Psychology is the science of finite individual minds" (Allport, 1937, p. 549); and that Titchener defined psychology as "The sum-total of human experience considered as dependent upon the experiencing person" (1924, p. 9). Despite the reference in these definitions to "subject," to "individual minds," and to "the experiencing person," the striking fact of modern, and especially of experimental, psychology has been the almost complete lack of concern with or study of the "subject," the "individual," or the "person." Stern, defining psychology from the personalistic standpoint, in language very similar to that employed by those quoted above, as "the science of the person having experience or capable of having experience" (1938, p. vii), has actually kept the person as a central and very real concept in his psychology. The person is not lost from view in the development of his system; instead, what Stern calls "the immediate subject matter of psychology, experience" is, in practice as well as in theory, "identified and interpreted in terms of its matrix, the unitary, goal-directed person" (1938, pp. vii-viii).

Textbooks of personality, in contrast to general psychological texts, have come upon the stage only within the last twenty-five years, and the major ones among these may still be very nearly counted on the fingers of two hands.[1] One seeks in vain, however,

[1] The authors of these texts, listed in the order of appearance of their books are Gordon (1926), Roback (1928), Allport (1937), Stagner (1937), Murray (1938), Thorpe (1938), Young (1940), J. McV. Hunt (ed.) (1944), Cattell (1946), Symonds (1946), Murphy (1947), and Harsh and Schrickel (1950). It should be noted that this is a list of major textbooks only: there has been, in recent years, an increasing number of reports of clinical, experimental, and clinicoexperimental studies of personality, proposals for new theoretical foundations for a theory of personality, collections of readings in the fields of personality and of personality and culture, etc. Reference to these will be found in the text of the present chapter.

to find in textbooks of personality any mention, to say nothing of an adequate treatment of, the traditional topics of general psychology. Murphy (1947) has come the nearest to incorporating within a single treatment of personality some of the topics of general psychology, but his attempt at integration is only a beginning; the major task is still to be done.

Historically there have been many reasons why personality was, for so long, a current apart from the main stream of swelling psychological theory. In an earlier day the problems of personality were largely conceptualized as philosophical in character; in a later period they were incorporated within the framework of religious and theological speculation; later still, when scientific investigation was coming into its modern ascendency, the problems of personality claimed the attention not of the psychologist but of the physician, and especially of the psychiatrist. Only in more recent years have the problems of personality found a grudging acceptance within a psychology that has prided itself on being scientific and which has insisted that its researches be restricted to problems susceptible of rigorous investigation.

Personality, treated for so long as an outcast from psychology and still regarded by the more conservative orthodox experimentalists as a marginal topic, appears to some as a parvenu claiming for itself and actually gaining, in its newly won position, a disproportionate amount of the interests and energies of psychologists. But such has been the course of the scientifically upward mobile concept of personality. If personality is not fully accepted or completely integrated into psychological theory, it has at least arrived.

The Science of Psychology and the Problem of Personality

The neglect of personality by a psychology that was seeking to become a truly experimental science is certainly understandable. When Wundt founded the Leipzig laboratory in 1879 there were techniques available for the study of sensory processes but none for the investigation of the person as a whole. Concerning partial processes of consciousness and response, specific and delimited ques-

tions could be asked and, one could hope, be answered experimentally. Despite the fact that an interest in personality and the cognate topics of character and temperament was as old as the interest in any other problem of psychology, a willingness to turn aside from problems of personality appears to have been a precondition for the development of an experimental psychology.

Already before Wundt, psychology's neglect of personality existed, and for some it had already become a matter of concern. As early as 1843 John Stuart Mill had complained that, while psychology, based upon the principles of association, had made the processes of mind "as plain as the road from Ludgate Circus to St. Paul's Cathedral" (in the words of his father, James Mill), it had left character entirely out of the picture. To correct this unhappy state of affairs, he championed a new science, Ethology, which, although it was to be based upon the general and abstract science of psychology, was to have, as its special object of study, human character (Mill, 1851, p 441).

In 1858 Samuel Bailey, troubled by psychology's preoccupation with general laws of consciousness and its neglect of the study of persons, urged the recognition of a new science of Individual and Personal Character as separate and distinct from Psychology (Bailey, 1858, p. 265).

On the continent a similar plea was made by Julius Bahnsen in 1867, who proposed that the new science be called Characterology. The characterologies that were developed after Bahnsen's work, like those which over the years had been appearing ever since Theophrastus wrote his famous *Characters*, were sometimes close to psychology but more often conceived to be distinct from it On the continent a tradition of characterology apart from psychology grew up, but in England when characterology was championed it was more often conceived, as, for example, by James Ward (1918), as an adjunct to general psychology. Even Wundt proposed the establishment of "a practical psychology, namely, a characterology, which should investigate the basic and typical forms of individual character with the aid of principles derived from a general theoretical psychology" (1895, Logik, II, 4, 1, (3), (b), p. 64).

Other proposals for a science of personality and character to deal explicitly with the problems of human individuality have been made under other names. Charles Mercier (1911, p. viii) suggested that it be called Praxiology, while Jan C. Smuts (1926, p. 279) argued the merits of a new discipline which he proposed be called Personology.

But none of these came to fruition. One may wonder why. The major reason seems to have been that all of these proponents of a science of personality either implicitly or explicitly conceived the problem of human individuality to lie outside the domain of psychology. True enough, the psychology of their day had neither the concepts nor the methods to deal adequately with the psychological problems of personality which seemed to them so vital. It was not, however, the inadequacy of the psychology of their day, but, as they saw it, its very excellence that led these men to despair of psychology ever coming to grips with the problem of human individuality. Their attitude was curiously ambivalent: psychology could not undertake the study of personality because it had already become so proficient in the investigation of another topic- the generalized, normal, human, adult mind. None of these proponents of a new science had either the imagination to see that psychology could grow and change or the courage to suggest that the problem of personality lay properly within the domain of psychology, to say nothing of the temerity to insist that personality was, in reality, its central problem. And since sciences are not established by fiat, but come into being through scientists extending their interests and their researches to previously neglected aspects of their fields, the sciences of ethology, individual and personal character, characterology, praxiology, personology, and others of like kind, died aborning.

This does not mean, however, that the place of the scientific study of personality has been simply or satisfactorily answered. The controversy still goes on. Specifically we may ask: "What is the relation of the psychology of personality to psychology in general?" and hope that in answering the question we may discover some of the major theoretical problems involved in the description, conceptual representation, and measurement of personality.

Historically the roots of the study of personality lie deep in many disciplines. But the same may be said of psychology in general.

The Logic of Schools of Psychology.—Table 13.1 presents a graphic attempt to bring order out of the chaos that has seemed at times to have been created by the schools of psychology. In originally drawing this table, in a simpler form it was Rosenzweig's (1937) intention to demonstrate a logical sequence in the development of schools which, in the clash of theories, has been too often overlooked. Reading from left to right, one moves, in general, from the earlier to the later established schools of psychology and observes in this history a mounting willingness and courage to tackle problems of increasing complexity. The progression of schools—if one looks at the central problem of each—has moved from a concern with the more static to the more dynamic problems of psychology, and from the more molecular to the more molar forms of behavior. The sweep of psychological problems and of the schools which have emphasized each of them is most clearly observed, however, when these schools and their core problems are seen in relation to other disciplines. The investigation of each of the major problems of psychology has revealed certain correlations between these phenomena and those of other sciences. But these correlations and the coordinations of the various schools of psychology with other sciences have been different for different problems and for different schools.

The schema of Table 13.1 suggests a logic in the history of schools, namely, that each was developed to deal with an area or problem of psychology which had been neglected by or arbitrarily excluded from the proper ("scientific") domain of earlier schools. In this sense, each new school of psychology has been a partial complement to its predecessors. Together they constitute, at least potentially, psychology; but until a common and single conceptual language can be agreed upon by all of them there will be partial psychologies rather than *a* psychology, and the relation of the theory of personality to the theory of general psychology will remain uncertain.

TABLE 13.1.—THE COMPLEMENTARY PATTERN OF PSYCHOLOGICAL SCHOOLS

				Functionalism		Geisteswissenschaftliche or Verstehende Psychologie
Name of School	Structuralism	Gestalt Psychology	Behaviorism	Purposivism	Psychoanalysis	
Characteristic problem or field of research	Sensation	Perception	Learning	Motivation	Personality	Value
Temporal span of observation in typical methods	.	C	→	⟶	⟶	⟶ ∞
Typical methods	Analytical Introspection	Phenomenological Introspection	Puzzle Box Conditioning Maze	Field Method of Observation	Case History Analysis of Dreams Free Associations Transference	Historical Research
P H Y S I C A L —		B I O L O G I C A L		— S O C I A L		
Conceptually allied sciences E.g.,	Physics		Physiology Neurology	Anthropology Sociology		History Economics Science Political
Leaders	Wundt Titchener	Wertheimer Köhler Koffka	Watson Tolman Hull	McDougall	Freud Adler Jung	Spranger

(After S. Rosenzweig, *Phil. Sci.*, 1937, 4, 96-106. By permission of the Author.)

There is a sense in which Table 13.1 should be extended to the left to show that prior to the firm establishment of Wundt's and Titchener's existential psychology, and for a period in rivalry with it, there existed a psychology, or, more properly speaking, a number of psychologies which, although they did not use the term personality, nevertheless treated personality, conceptualized as the self, as a central problem.

With this extension of the chart in mind, even though it is not appended to Table 13.1, we may picture the history of psychology and of psychological theory in the modern period as, in large part, the result of a basic and prolonged conflict between those psychologists who, on the one hand, have insisted that psychology model itself as much as possible after the physical sciences and those who, on the other hand, have argued that the acceptance of any such goal would rule out of psychology its most unique and most valued datum, the self. When, in a still earlier period, psychology had liberated itself from theology it had freed itself of the concept of a soul; now, toward the end of the nineteenth century, those who set out to develop a new experimental psychology, firmly grounded in physiology, sought to purge the language of psychology of such words as self and ego and person and to exclude from psychology the problems which through the years had been formulated in these terms. Others, less rebellious against the philosophy which had mothered their science, and often more at home with empirical than with experimental method, saw no reason why the self should not remain an object of study of psychology, and, indeed, every reason why it should. It may be said of the former group that when they were forced to choose between the rival claims of parsimony and completeness (in the sense of range and complexity of data encompassed) of theory and method they chose parsimony, while the latter group were on the side of completeness.

Personality and the Theory of Science

The theory of personality is not unrelated to the theory of science. Indeed, the clash of opinion concerning theoretical problems in the field of personality is, in two important instances, only an expression of a difference of opinion concerning the nature of science.

The first of these controversies centers on the question as to whether the task of the student of personality is to *describe* or to *conceptualize* personality or possibly to do both. The answer to this question is dictated accordingly as one sees science as properly concerned only with data, with fact-finding and fact-description, or conceives science as properly moving from data to the conceptualization of hypothetical constructs or intervening variables in terms of which data can be represented and from which deductions not otherwise derivable can be drawn.

The second of the controversies arises out of two opposed reactions to the irrefutable fact of the individuality of every person and the traditional acceptance of the age-old dictum, "Scientia non est Individuorum" (Allport, 1937, p. 3). In the one camp are those who, accepting the fact and the maxim, assert that individuality cannot be an object of scientific study. In the opposing camp are those who insist that, if science as usually practiced is not able to deal with the fact of individuality, then a new science adequate to the task must be created.

Science and the Individual.—The problem, as seen from the point of view of one who doubts that science is capable of dealing adequately with the unique phenomenon which every person is, has been stated by Smuts as follows:

> "Each human individual is a unique personality; not only is personality in general a unique phenomenon in the world, but each human personality is unique in itself, and the attempt at 'averaging' and generalizing and reaching the common type on the approved scientific lines eliminates what is the very essence of Personality, namely, its unique individual character in each case. The scientific procedure of psychology, inevitable as it is for psychology as a scientific discipline, is not very suitable in respect of a subject so specially individual as Personality" (1926, p. 279).

The reader who may be inclined to criticize Smuts' statement on the grounds that it presents a too simple exposition of the method of science should nevertheless recognize that it is a clear expression of an attitude that had led, even before his day, to a bifurcation of the approach to the study of personality. This bifurcation was

most explicitly and most systematically developed on the continent, where it took root in and was nourished by the philosophers' concern with the method of science. The result was not only a bifurcation of psychology into *Erklärende Psychologie* (a psychology of explanation) and *Verstehende Psychologie* (a psychology of understanding), but also a more basic dichotomizing of science into *Naturwissenschaft* (natural science) and *Geisteswissenschaft* (mental or cultural science).

Verstehende Psychologie.—It was the German philosopher, Wilhelm Dilthey, and his pupil, the psychologist Eduard Spranger, who contributed most significantly to the distinction between a *Verstehende Psychologie* as *Geisteswissenschaft* and an *Erklärende Psychologie* as *Naturwissenschaft.*

Dilthey's programmatic paper—"Ideen uber eine beschreibende und zergliedernde Psychologie"—appeared in 1894 (Dilthey, 1924). The distinction which he drew in this paper between a "descriptive" and an "analytical" type of psychology came eventually to be formulated as the distinction between a psychology of understanding and a psychology of explanation.

Dilthey's concern was to rescue psychology from the barren futility of brass instrumentation, physiological bias, and minute analysis of mental life into which, so it seemed to him, it had fallen under the aegis of Wundt. In his belief that the psychology of his day was neglecting the truly significant problems of mind for the sake of winning an acceptable scientific form, Dilthey was joining the company of those who, through the ages, have believed that psychology should be concerned with life as it is lived, i.e., with life experienced with value. For the study of such problems, the techniques of natural science, he argued, are inadequate; a different scientific methodology is required.

As Dilthey saw it, the concern of psychology should be with character or the value-aspect of mind. Its aims should be to achieve an *understanding* of those facets of mind which are responsible for the appearance and development of what has been variously designated as civilization, culture, the life of the spirit, humanism. For an understanding of these aspects of mind, academic psychology,

traditionally empirical and, in his day, becoming experimental, had, Dilthey believed, little to offer.

Basically, Dilthey was attacking the notion that the analytical and reductive methods of the inorganic sciences could be applied to the problems of mental life. As a special object of his attack he took the "explanatory" psychology of Ebbinghaus,[2] which, as he saw it, was modeled after the ideal of an atomistic physics and suffered from the fact that it consisted primarily of hypotheses. In its place, he argued, there was need for a "descriptive" psychology which would serve as a foundation for all the cultural and social sciences.

Dilthey's anti-elementaristic position was clearly formulated in his statement that "By *descriptive* psychology I understand the representation of the components and connections of each mature human mind as they are united in a single tie, which is not added by reflection or deduction, but is directly experienced. This psychology is a description and analysis of a relation which is as primitively and directly given as life itself" (1924, p. 152).

In this passage one sees, as Hartmann (1935) has pointed out, an adumbration of a point of view which was later to become crystallized in Gestalttheorie, although none of the founders of Gestalt psychology has ever recognized Dilthey as a spiritual progenitor. And for good reason, for while Dilthey was, like them, critical of an atomistic analysis of mental life, he was equally critical of the experimental approach to psychological problems which has characterized the investigations of Gestalt psychologists. Yet one should not for this reason overlook the fact that much that Dilthey wrote could have served as well as a philosophical basis for all schools which have stressed structure and the givenness of wholes as it did for the justification of a *Verstehende Psychologie*. Unfortunately, however, Dilthey overstated his case and emphasized differences where similarities might also have been noted. Thus his actual contribution was to a bifurcation of psychology and of scientific method, rather than to the unification of science.

The descriptive psychology which Dilthey championed was to be a useful one, one which would serve as the basis for the cultural

[2] For Ebbinghaus' reply, see his "Ueber erklarende und beschreibende Psychologie," *Zeitschr. f. Psychol.*, 1896, 9, pp. 161-205.

and social sciences just as mathematics provides the necessary foundation for the natural sciences. Such a psychology, descriptive of mental life, could not, he argued, be achieved by the techniques of an "analytical" psychology, namely, introspection and laboratory experimentation. The true nature of man's mind was rather to be experienced through a process of understanding.

Dilthey conceived understanding to be a process by which one experiences every part as always embedded in an articulated whole. In so far as one is constantly aware of the reference of parts to their larger wholes, for example, the relation of any single bit of behavior to the totality which is the individual, one is engaged in the process of understanding. The experience of articulated wholes, *Strukturzusammenhangen*, was, according to this view, the method of a descriptive mental science. Such a psychology would start, not with elements between which causal connections would be constructed, but with the experienced, inner connections of the *Strukturzusammenhangen*.

In Dilthey's writings were the seeds of certain aspects of both Gestalt psychology and the clinical psychology of the individual which were to come later, but they never took firm root, for Dilthey insisted upon throwing them upon a soil outside the domain of empirical science. And paradoxically, although Dilthey proposed that the individual be elevated to a position of central interest in the new descriptive psychology, in its development the individual was lost from view and the psychology which was to have been useful became instead a highly abstract typology, for the method proposed by Dilthey was not the empirical and experimental investigation of individual men, but an examination of man's history.

It was not Dilthey, the philosopher, however, but Spranger, the psychologist, who determined the specific form which *Verstehende Psychologie* was to acquire. Dilthey laid its methodological foundation, but Spranger actually developed it.

There is a peculiarly contemporary ring to Spranger's early insistence that psychology "must start from the personality as a whole as it stands in intimate contact with an historically developed cultural environment" (Kluver, 1929, p. 447). This is the core

assumption which all researchers in the area of "personality and culture" have made since 1925 when this area of overlapping research of psychology and anthropology was formally named (White, 1925). Yet the methods and the findings of *Verstehende Psychologie* are almost polar opposites to those of the contemporary psychology of personality and culture.

Spranger's work, best exemplified in his *Lebensformen* (1925) which has been translated into English as *Types of Men* (1928), is best known as an exposition of six fundamental types of individuality, each centering on a different value or meaning. The major characteristics of Spranger's types are well known, if not in their full original German exposition at least in their condensed English versions (Vernon and Allport, 1931; Allport, 1937). Less well known, however, is Spranger's rationale of the ideal types of values and of men which he has conceptualized as (1) the theoretical, (2) the economic, (3) the esthetic, (4) the religious, (5) the social, and (6) the political.

Spranger sought to develop a psychology that would be adequate to deal with those aspects of experience which man can communicate to and share with others. Such partial psychological processes as images, feelings, drives, etc.—the object of study of what he called "the atomistic psychologies"—are, he argued, meaningless, inexplicable to others and, of necessity, purely subjective. His *Struktur* psychology, in contrast, would concern itself with the *meaning* of subjective experiences. Such meanings alone can be communicated to others and objectified in such cultural products as science, art, religion, technology, etc. The task of his *Verstehende Psychologie* would be to discover and to describe the distinguishable categories of acts (*Geistesakte*) through which meaning is objectified or expressed in the historically developed cultural environment in intimate connection with which the individual always stands. These *Geistesakte* he designated as scientific, economic, esthetic, religious, sympathy, and mastery acts and from them he conceptualized his six well-known ideal types of meaning or value.

Although Spranger tells us that he arrived at these fundamental types by "the study of history and faithful daily observation," he

also cautions us that these types cannot be empirically identified.[3] They are rather categories or schemata of understanding by reference to which the complex types which we experience can be understood.

The importance of Dilthey and Spranger and of *Verstehende Psychologie* and *Geisteswissenschaft* for the study of personality lies not in the adequacy of the answers which they gave, but rather in the vividness with which they formulated certain basic theoretical and methodological questions. The questions which they raised have been variously answered by those who preceded them as well as by those who have followed after them. One may criticize the specific answers given by Spranger's particular form of *Verstehende Psychologie*, yet at the same time see in them a partial corrective of the formulations of those who would neglect entirely such difficult problems as those of individuality and human values.

As between the adequacy, for the conceptual representation of personality of a psychology which would deal only with elements and one which assumes structures, there seems to be increasingly little doubt that the latter is superior. On the other hand, it is questionable whether Dilthey and Spranger were correct in insisting that there is no common ground between a psychology of elements and a psychology of structure. Such concepts as those of syndrome, general and group factors of intelligence and personality, habit and need hierarchies, etc., are hardly meaningful without the notion of some sort of elements entering into a more or less enduring structure.

Although the method of *Verstehen* has been criticized for leading, in the hands of Dilthey and Spranger, to the development of highly abstract typologies, there is no reason to believe that such abstract typologies are the inevitable product of *Verstehen*. Spranger's definition of *Verstehen* as the mental activity that "grasps events as fraught with meaning in relation to a totality" (1927, p. 148) serves equally well to define the process whereby the clinical psychologist and therapist comes to understand the meaning of very concrete manifestations of human individuality. And there is nothing in the

[3] This caution has fortunately been disregarded by G. W. Allport and P. E. Vernon (1931) whose test, *A Study of Values*, has been devised for the purpose of determining the pattern of Spranger's values in an individual.

definition to preclude an integration of the application of general laws with the observation of a concrete case. The claim for the irreconcilability of *description* and *analysis* and of *understanding* and *explanation* has not been proved. On the other hand, the theoretical and methodological problems raised by these opposites have not as yet been satisfactorily solved If and when they are, the day of psychological schools will be over; there will be in their place a single systematic psychology.

The conflict between *description* and *analysis* and between *understanding* and *explanation*, brought into sharp focus by Dilthey and Spranger, was also of great concern to the philosopher Windelband (1894). His contribution, however, like that of Dilthey and Spranger, was not to reconcile the opposites but to make their differences even more clear-cut For the two opposed types of science Windelband proposed new terms, the *nomothetic* and the *idiographic* The natural sciences, seeking to establish causal laws of the greatest possible generality, he designated as nomothetic The social or cultural sciences, employing the methods of history in their investigation of unique events, he proposed should be called idiographic Having distinguished nomothetic and idiographic methods, he insisted that the latter had fully as much right as the former to be called "scientific." The methods of history, he believed, have the same scientific dignity as the methods of the physical sciences He then proceeded to point out that psychology had been seeking to become, like the natural sciences, a nomothetic science, but that there was equal reason for it to develop into an idiographic science which, like history, biography, and literature, would attempt to understand particular and unique events Such an idiographic psychology would have as its object of study human individuality

The distinction drawn by Windelband between nomothetic and idiographic methods, and his championing of the latter as being as fully scientific as the former, have been an encouragement to many psychologists to pursue their investigations of individuality without concern for general lawfulness. For others—notably Allport (1937) and Lewin (1935)—the dichotomy of method is too sharp. Allport, emphasizing the individual, wants also to pay some attention

to general laws, especially as they may be established within the individual; Lewin, emphasizing general lawfulness, has been concerned to point out how one proceeds from the general law to the individual case and from the individual case to the general law.

Allport, in calling our attention to the fact that over half a century ago, Azam, the French psychiatrist, wrote that the science of character "cannot proceed by generalities, as does psychology, nor by individualities, as does art. It occupies an intermediate position" (1887, p vi) adds his own opinion that "this intermediate position will fall properly within the scope of a *broadened* psychology" (Allport, 1937, p 23) That "broadened psychology" with an "intermediate position" has not yet arrived, however clear the signs that it may be on its way There are still varied approaches to the problem of individuality and for that reason this chapter

Description vs. Conceptualization of Personality.—Whether, in the study of personality, one should limit oneself to a description of the phenomena of behavior, or, moving beyond data, should develop hypothetical constructs or intervening variables is as we have already seen (p 610), the other basic problem in the field of personality which is rooted in the philosophy of science This question arises from a conflict of two basic conceptions of scientific methodology positivistic induction *vs* hypothetical deduction

Some prefer to describe personality as a set of overt and manifest behaviors, others prefer to define it as a set of hypothetical constructs in terms of which actual behavior is to be explained This opposition is also clearly reflected in the etymology of the terms *persona* and personality The history of the meanings which these terms have acquired from the days of the early Romans to the present has been reviewed by Allport (1937)[4] He was able to discover fifty different meanings, yet was also able to note in them a frequent recurrence of two basic and fundamentally opposed formulations the outward, superficial appearance *vs* the inner, essential nature of man The former has been designated the *mask*, the latter the *substance* definition of personality.

[4] For other treatments of this topic, see MacKinnon (1944) and Bayet (1948).

Those who have offered mask definitions have tended to think of personality in terms of either stimulus[5] or response.[6] Such conceptualizations of personality are found characteristically in the writings of those of a behavioristic persuasion. They appeared most frequently in the writings of American psychologists in the score of years from 1910 to 1930 when behaviorism was riding high. They reflect the positivistic, objective, descriptive interests of those who, influenced by Watson, were reacting strongly against anything that might be considered subjective, hypothetical, or abstractly conceptual. Such parsimonious and phenotypical definitions of personality have always had, however, a special attractiveness for the peripheralists in psychology.

Substance definitions of personality, on the other hand, have throughout the years more often appealed to those who have not hesitated to turn their attention to the problems of personality in fields other than that of a narrowly conceived and positivistic psychology. And indeed, many of those who have most clearly conceived of the personality as referring to some reality behind the phenomenal appearance of man have been workers, not in psychology, but in such related fields as philosophy, law, medicine, anthropology, and theology. For such authors, whether psychologists or not, personality is not a psychological datum, but a concept, constructed in the minds of men for the purpose of explaining the behavior, both subjective and objective, of individual persons.

So rich in content are the substantive conceptualizations of personality apt to be that frequently their authors do not even attempt to compress them into brief definitions. As most notably in the case of Freud and the psychoanalysts, one has to read their articles and their books to discover in what sense they use the term personality. As an illustration of a brief substantive definition, however, the following will suffice: "Personality is the entire mental organization of a human being at any stage of his development" (Warren and Carmichael, 1930, p. 333).

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[5] E.g., "It is the responses made by others to an individual as a stimulus that define his personality" (May, 1932).

[6] E.g., "Personality is the characteristic behavior of an individual" (Sherman, 1929, p. 174).

The distinction between mask and substance definitions of personality parallels closely that which has been drawn between *biosocial* and *biophysical* conceptions of personality (Allport, 1937). According to the biosocial view, a man's personality is identified with what he appears to others to be, i.e., his social stimulus value. There is at least implied in this view the notion that a person alone and by himself has no personality; he acquires one when he is perceived as a person by others and loses it when he withdraws from them. According to such a view Robinson Crusoe—until the advent of Friday—was without personality.

In contrast to the biosocial view stands the biophysical conception of personality which asserts that "personality, psychologically considered, is what an individual *is* regardless of the manner in which other people perceive his qualities or evaluate them" (Allport, 1937, p. 40). This is the view that Allport favors. His own definition of personality is simply stated, "what a man really is," or, more formally expressed, "Personality is the dynamic organization within the individual of those psychophysical systems that determine his unique adjustments to his environment" (1937, p. 48).

Although the first part of this definition clearly expresses the biophysical conception of personality, the last phrase implies some measure of biosociality, for if uniqueness of adjustment is to be known it must be perceived by another and compared with the modes of adjustment made by others. And indeed, despite the ease with which mask or biosocial conceptions of personality may be distinguished from those which stress the substantive or biophysical nature of personality, it is not at all uncommon to find formal definitions of personality which encompass, even though they may not integrate, both points of view.

Furthermore, while Allport's exposition makes it clear that mask or biosocial definitions of personality employ the language of data, the nature of that to which substantive or biophysical definitions refer is not so clear. At times it would appear that such definitions refer to data, although not those usually investigated by psychologists, while in other contexts substantive or biophysical definitions clearly refer, not to data, but to conceptualized entities or processes.

Lewin (1931) has argued that much of the confusion, not only in the field of personality, but in psychology in general, has been due to a failure to distinguish clearly two kinds of scientific language: the phenotypical language of data and the genotypical language of constructs. Lewin's position was not, as many seem to have assumed, that genotypes alone should be employed, but that one should be clear about when he is referring to phenotypes and when to genotypes.

The confusion of these two languages by the psychologist working in the field of personality has arisen, in part, from his eagerness to leave nothing aside, to include within his concept of personality the content of the whole field of psychology if not also that of all the biological and social sciences. This attempt to make personality all-inclusive has led workers in this field to offer, upon occasion, not one but several definitions ranging from the more temporally and spatially constricted to the more temporally and spatially extended conceptualizations of personality.

Among recent investigators who have felt impelled to offer not one but a series of definitions of personality have been Murray (1938) and Murphy (1947).

Murray has proposed that three meanings of personality be distinguished, each expressed, however, in terms of his basic concept of regnancy as "the organized aggregate of mutually dependent processes which momentarily determines the functioning of the organism as a unit." Since the regnancy is clearly not a datum but a hypothetical concept, Murray's definitions of personality are cast in the language of constructs.

The *momentary personality* is conceptualized as "the configuration of the regnancies during one event in a subject's life."

The *life-time personality* may be considered as "the historic route of such configurations from birth to death."

The *common personality* is defined as "the most commonly repeated configurations during any period of an individual's existence" (Murray, Lectures).

In his attempt to do justice to the richness of meaning which the term personality has acquired not only in our common language but also in scientific usage, Murphy (1947) has proposed three defini-

tions of the term, the first of which is phenotypical, while the second and third refer to genotypes:

1. "A personality is a distinguishable individual, definable in terms of a qualitative and quantitative differentiation from other such individuals."

2. "A personality is a structural whole, definable in terms of its own distinctive structural attributes."

3. "A personality is a structured organism-environment field, each aspect of which stands in dynamic relation to each other aspect. There is organization within the organism and organization within the environment, but it is the cross organization of the two that is investigated in personality research" (Murphy, 1947, pp. 7-8).

It may be pointed out in respect of these as of most definitions that they do scant justice to the author's full conceptualizations of personality. For example, although there is no mention of the temporal or developmental aspects of personality in Murphy's formal definitions, these topics receive full attention in his writing.

Little would be gained by listing here all the definitions of personality which have been offered not only by psychologists, but by workers in other fields as well, and placing them in categories: mask *vs.* substance, biosocial *vs.* biophysical, phenotype *vs.* genotype, for this has been done elsewhere (Allport, 1937; MacKinnon, 1944). Having noted these opposed categories of definitions as an expression of the basic conflict over the place of description and conceptualization in the psychology of personality as well as in science more generally, it will be more profitable to note other basic conflicts in personality theory, conflicts which arise, however, not from differences of opinion about scientific method but from differences of opinion about the nature of personality and its determinants. But first a bit of history.

The Constitutional Approach to Personality

In antiquity, as well as in the modern period, the physician preceded the psychologist in coming to grips with the problems of personality.

The physician's interest in personality arose originally out of his attempt to discover the role of constitutional factors in disease

Often, it seemed, patients were characterized by a disposition to develop one type of illness while carrying what looked like an immunity to other diseases. And frequently it appeared that differences in disease potentiality and immunity were correlated with psychological differences, most notably of mood and temperament. In seeking the biological basis of disease-potentiality and immunity they were, if only incidentally and implicitly, investigating the bodily correlates, if not causes, of psychological states. The distance they moved toward a real understanding of personality was, however, regrettably short.

The training of the physician, in an earlier day, was entirely in biological science and even today remains largely in that field. The wise physician may come to think of his patients as persons and of man as a historical being as well as a mammal, but these are not attitudes which medical schools spend much time inculcating in their students. It is, then, not surprising that medical men have tended to treat the psychological facet of their patients as an epiphenomenon to be noted, or at most as the dependent variable in their investigations.

Personality and Physiological Factors.- It is to be said of the doctors, however, that they were the first to propose physiological explanations of differences in personality, and it was no less a person than Hippocrates, the so-called father of medicine, who started this tradition. Breaking with the magical and religious notions which characterized the physician's lore of his day, and building upon Empedocles' doctrine of four cosmic elements, Hippocrates developed a theory of pathology which sought to explain both human disease and human temperament in terms of varying admixtures of assumed bodily humors.

Hippocrates' attempt to find the cause of individual differences in characteristic response and temperament in bodily humors set a pattern of thought which has continued from his day to the present. It is one of seeking the explanation of more complex phenomena (in this case, mood and temperament) in phenomena considered in some sense simpler or more elementary and basic. It is characteristic of such a view to assume a hierarchy of the sciences and to believe that, although the data of any science may be described in the lan-

guage of that science, their explanation must be sought in the language of sciences which are more basic.

In a terminology which has been frequently used, the data of the super-organic sciences (e.g., anthropology and sociology) would be explained by reference to the data of the mental organic sciences (e.g., psychology), whose data in turn would be explained by the organic sciences (e.g., physiology, neurology, etc.) whose data could be explained only on the level of the physical sciences of chemistry and physics.

Within the limits of this chapter it is obviously impossible to list by name or even to designate by categories all of the researches into personality the rationale of which has been to explain phenomenological and behavioral data by reference to the facts of those sciences which are conceived to be basic to or supportive of the psychology of personality.

From Hippocrates to the present day there have been repeated attempts to explain differences in personality and temperament and in some cases even of character by reference to fluids of the body. By Hippocrates they were called humors, but the modern endocrinologist thinks of them as secretions of the ductless glands and refers to them as hormones. The early history of man's attempt to develop what might be called a humoral psychology has been traced by several authors (e.g., Stern, 1911; Pillsbury, 1929; Roback, 1927, 1931; Klages, 1932; and Irwin, 1947). The contributions of modern endocrinology to problems of the psychology of personality have been presented both extravagantly (Berman, 1921; Cobb, 1927) and conservatively (Lipschutz, 1925; Rowe, 1932; Hoskins, 1933, 1941, 1946; Shock, 1944; and Loeb, 1945).

Personality and Physique.- A line of thought parallel with and overlapping to some extent that which has just been described has been one that has seen personality and physique as functionally related. By some the relationship has been thought to be determined by the fact that both are influenced by biochemical and hormonal states, while others, more psychologically and culturally oriented in their thinking, have stressed the fact that idiosyncrasies of personality may result from a person's perception of and emotional reaction to peculiarities of his body-build and the evaluation placed by the

individual's culture upon the kind of body he possesses and the kind of person he is.

The search for morphological correlates of personality began, as had the inquiry into its physiological bases, with Hippocrates, and both for the same reason: to discover more about disease potentiality and immunity. Hippocrates described two opposed types of body-build, one long and narrow, the other short and thick. The former, he believed, was disposed to develop respiratory disturbances, and for that reason he labeled it *habitus phthisicus*. The latter, prone, he thought, to suffer from disorders of the circulatory system, he designated *habitus apoplecticus*. Those who have followed in the tradition set by Hippocrates have, by and large, developed dichotomous typologies of human physique. Although each investigator has proposed a different terminology and many have found correlations different from those reported by Hippocrates, the types of physique have, in the majority of instances, been those first described: long-narrow *vs.* short-thick. Some have felt the necessity of distinguishing three types of body-build, and a few have differentiated many more.[7]

So long as interest was centered on the relation of constitutional type to physical disease potentiality and immunity, the resultant typologies were tangential to the psychology of personality, but when, through the work of Kretschmer (1925), interest shifted to the relation between bodily type and predisposition to mental disorder, constitutional typology acquired a central position in the psychology of personality.

Neither the reported findings of Kretschmer nor the criticisms of his work need be reviewed here, but his tremendous influence upon constitutional research must be noted. It was not so much that his findings were original—the relationship which he documented had been long observed[8]—but that they came at a time when the intel-

[7] For a review of the literature on constitutional types, see Sheldon (1940).

[8] Three hundred and fifty years previously, William Shakespeare had written:

"Cæsar. Let me have men about me that are fat,
Sleek-headed men, and such as sleep o' nights:
Yond Cassius has a lean and hungry look;
He thinks too much: such men are dangerous.
"Anthony: Fear him not, Cæsar; he is not dangerous.
He is a noble Roman, and well given.
"Cæsar: Would he were fatter!"

lectual climate was ready to receive them. Crude as his methods were, both for the determination of type and for the statistical treatment of their occurrence, here, at least, was something more than subjective impression and a relating of the concept of type beyond morphology to temperament and psychopathology. One aspect of Kretschmer's work, frequently overlooked, was his introduction of the concept of dysplasia or mixed type. And this was important because—as Sheldon has noted—it led naturally to the notion of "continuous distribution and measurable elemental components *instead* of types" (1940, p 530), a notion which was to exert great influence on Sheldon's own investigation of *The Varieties of Human Physique* (1940) and *The Varieties of Temperament* (1942).

Investigators, stimulated by the work of Kretschmer and Sheldon, have, in some cases, confirmed these authors' findings but in other instances have obtained negative or contradictory results. Sheldon (1940), reviewing the repetitions of Kretschmer's study, finds the majority reporting confirmation Of those who have sought to test Sheldon's theory of the relationships between somatotype and temperament, Child and Sheldon (1941) and Fiske (1944) obtained insignificant correlations, while both Coffin (1944) and workers in the Grant Study at Harvard find support for Sheldon's theory Heath (1945) believes his data indicate that personality is more closely related to type of body-build than to family background or social environment, while Seltzer *et al* (1948) report that extremes of somatotype and temperament are related, although the relation may be obscured in less extreme cases.

Many more studies will have to be made before any final verdict on Sheldon's theory can be given Investigators, in the future, should, however, be warned against seeking to test Sheldon's reported correspondences of somatotype and temperament by means of correlational studies, for, as Harsh and Schrickel point out

> ". . a given amount of one somatic component is manifested in many dissimilar behavior traits, depending upon the pattern of the other two components. For example, an endomorphic 5 in a 5-1-4 somatype would refer to a tall, soft, effeminate man who would probably be clumsy, inactive, and unsure of himself. But in a 5-4-1 somatype it would refer to a powerful, active, barrel-

> bodied man with enough drive and self-assurance to become a top executive. Thus one should rarely expect to find simple linear relationships between behavior traits and somatic components" (1950, p. 374).

Although Sheldon's somatotypic representation of the varieties of human physique has attracted most attention in recent years, it should not be forgotten that there are other approaches to the problem of body types, as, for example, the one through factor analysis (Thurstone, 1946).

In recent years the climate of opinion in psychology has become increasingly critical of constitutional researches. This, it may be noted, is in striking contrast to the situation which obtained in the early years of modern psychology when there was a disposition to believe that the major determinants of personality would be discovered by researchers in the biological sciences (see Table 13.1). One may wonder why the quest for determinants has, more recently, shifted increasingly to the social sciences.

In part, the shift has been due to the fact that the biological scientist—be he physiologist, neurologist, endocrinologist, or whosoever—has had, when all is said, so little to offer for the understanding of the complexly motivated, more or less normal behavior, of the person-as-a-whole. His contributions to an understanding of partial processes and especially of their pathological functioning have been great, but they have shed little light on the more complex problems of personality.

The relatively narrow focus of many investigations of constitutional and biological determinants of personality, leaving aside, as they have, any consideration of the role of the complex environmental factors of society and culture, has violated Stern's convergence principle and run counter to the growing disposition to stress the multiple causation of behavior which sees all response as determined not by local, single causes but by the multiple forces of the total field of which the individual is a part. The falling out of fashion of constitutional researches has been in part conditioned by the current field-theoretical climate in the psychology of personality

Actually, many of the well-established findings of biological and constitutional research have been, if not entirely overlooked, at least

not integrated into present-day attempts to develop a complete and systematic psychology. The reason for this neglect seems to lie in a characteristic bias of psychologists—a bias in part determined by the very subject matter of their science and in part by certain unrecognized or unformulated humanitarian ideals. This bias is one which has led psychologists to place in the center of their science the problem of change—and implicitly change for the better. In a very real sense, the central problem of psychology has become, if it has not always been, the problem of learning. Although the psychologist is not so rash as to deny that there are biological givens, hereditary Anlage, constitutional factors, etc., he has been content to consider these "unpleasant realities" largely as restraints upon the extent to which learning can occur and to leave their investigation to the biological scientist. If one questions the centrality of the problem of change in psychology, he may call the roll of the various guises in which the problem of learning has attracted the energies and interests of psychologists: memory (Ebbinghaus), trial and error learning (Thorndike), conditioned reflex (Pavlov), stimulus-response bond (Watson), sentiment (McDougall), insight (Köhler), etc. Conversely, psychopathology in its various guises has been reduced to a failure to learn or faulty learning: fixation, neurosis, maladjustment, rigidity, regression, and so one could go on. The growth and development of personality as a psychological problem constitute the problem of learning.

For the reasons just reviewed, as well as for many others, the earlier over-emphasis upon the role of biological and constitutional factors as determinants of personality has given way within the last fifteen years to an increasing preoccupation with such social and institutional determinants of personality as culture, social class structure, group-membership, social role, etc. Partly this shift has been caused by the novelty of method and theory in the area of personality and culture as well as by the success with which researches in this new area have been carried out, and in part it has been furthered by our very understandable desire to prosecute researches whose findings would refute such widespread belief in racism as found expression in Nazi ideology.

The Literary Approach to Personality

In the history of man's attempt to portray the essence and manifestations of human individuality novelists, poets, and dramatists have stood in a position of pre-eminence. None has surpassed the best of them in drawing individual characterizations and in depicting the springs of human action. Yet however penetrating their insights into human personality have been, the major contribution of these men of letters has been to the artistic delineation of individuality rather than to the scientific abstraction and conceptualization of character and personality.

There have been, however, writers who have used their literary skills to draw word portraits of what they have believed to be universal types of human being. These have been the so-called literary characterologists, and their contribution has been both to the fields of letters and to a typology of personality.

Literary characterology stems from antiquity. Plato in his *Dialogues* and Aristotle in his *Nichomachean Ethics* made minor contributions to the literary form of character writing, but it was Theophrastus, Aristotle's pupil, who, in his *Characters*, written when he was 99, really established this form of writing

In addition to Theophrastus, the name of Jean de la Bruyère shines brightly among characterologists (Aldington, 1924). Among their number are to be counted also such literary figures as Chaucer, Ben Jonson, Joseph Addison, Richard Steele, Samuel Johnson, George Eliot, Samuel Butler, to mention but a few. The full role of characterologists has been called by Roback (1927, 1928), while a briefer and more popular history of characterology has been prepared by Jastrow (1915).

However skillfully the literary characterologists have portrayed personality, their descriptions as well as the theory of personality implicit in them have been relatively superficial, stressing, as in Theophrastus' *Characters,* the importance of some one dominant trait which colors all that a person does, or emphasizing, as in La Bruyère's *Portraits,* the unique style which characterizes every man's behavior. Those who value especially the uniqueness of every person, and argue exclusively for the idiographic study of personality, will

remain forever dissatisfied with portraits drawn by characterologists since they represent types rather than individuals. To this, however, characterologists might well reply that their work is like that of the scientist who, in seeking to introduce order and simplification into the complexity of data, moves of necessity away from phenomena through the intellectual processes of abstraction and generalization. All too often, however, the types which the characterologists have portrayed for us have been overdone and drawn too uncritically.

Literary characterologists and constitutionalists have not been the only ones to offer typological systems for the classification of personalities. Typologies have also been proposed by psychiatrists, experimental psychologists, and psychoanalysts.

THE EXPERIMENTAL PSYCHOLOGISTS' APPROACHES TO PERSONALITY

The approaches to personality so far reviewed in this chapter have developed for the most part outside the domain of academic and experimental psychology, and, in the section following the present one, we shall note that the greatest impetus to the search for understanding of personality has, in the modern period, come not from the experimental psychologists but from the psychoanalysts. For the present, however, our concern shall be with the approaches to the study of personality which have developed within the psychological laboratory. These began in the modern period with the recognition of "the personal equation" (see Boring, 1929, Ch. 8), the fact of individual differences, and the attempt to develop methods which would permit of the conceptualization and measurement of such differences within the framework of a nomothetic and explanatory psychology.

British Experimentalists and the Investigation of Personality.--The predominant pattern of investigation which has characterized the work of British psychologists in their study of personality is one which was set by Francis Galton (1822-1911). The approach to personality which he established is one which in recent years has been called "elementalistic" in contrast to an approach

which has been designated as "organismic" (OSS Assessment Staff, 1948). This pattern of experimentation has had wide influence on psychological studies outside of as well as within Great Britain.

Let us see what it was that Galton did. First, he did not seek to study anything as complex as "the person-as-a-whole" or personality, but one aspect of it, namely, intelligence. Second, he did not attempt to measure intelligence directly, but rather to measure simple sensory and motor capacities in the belief that such measurement would, through correlations with, be indicative of an individual's intelligence and judgment. What was characteristic of Galton's approach and the approach of so many who have followed him, was its emphasis upon partial processes and segmental responses as indicators of the qualities of the larger wholes of which they are but parts. This elementaristic approach to personality is one which has had wide appeal for psychologists as being a more manageable and more objective approach to personality than is offered by what is frequently referred to as the organismic or holistic point of view.

The fact that Galton's hope that tests of simple sensory and motor capacities would yield reliable information about intelligence proved to be a vain one did not destroy the belief that the experimental and statistical methodology to which he contributed so much would prove some day to be the *via regia* to personality.

It is not possible to list all who have labored in Galton's tradition, but among those whose names stand out in the attempt to establish relationships between simple or at least partial functions and more inclusive aspects of personality, whether it be intelligence or character or type of personality, are Spearman and his co-workers, Wynn Jones, Aveling, Carey, Flügel, Lankes, Bernstein, *et al.* (Spearman, 1927).

If there has not been a British school there has at least been a British tradition in the experimental approach to personality. Investigators in this tradition show the continuing strong influence of Darwin and evolutionary theory in their predilection to stress and to study most intensively the biological, hereditary, and constitutional aspects or determinants of personality to the relative neglect of social and cultural factors. They have emphasized the units or elements

of behavior—the simpler or, at the most, partial processes or functions—which they have investigated in as rigorous and objective a fashion as possible. They have contributed significantly to the development of statistical methods, and through the utilization of these methods in the analysis of their experimental data they have sought to establish various factors of intellect, temperament, character, and personality (Spearman, 1904, 1927; Burt, 1941, 1945; Thomson, 1939; Stephenson, 1936a, 1936b; R. B. Cattell, 1946; Eysenck, 1947).

There have been, to be sure, psychologists in Britain outside of this tradition. One thinks of Ward, Sully, Stout, Shand, and McDougall, but their contribution, like those of a small but active group of psychoanalysts and medical psychologists in England, has been to the conceptualization of the complexly motivated manifestations of personality rather than to the establishment of factors through the analysis of a matrix of intercorrelations of test scores. In their treatment of personality, they have operated for the most part as systematic psychologists rather than as experimentalists.

American Experimentalists and the Investigation of Personality.—The British tradition has been paralleled by a movement, although not a school, among the many streams in American psychological theory and research in the area of personality. This began with James McKeen Cattell who, even before he went in 1880 to study with Wundt, was convinced of the importance of individual differences. In Wundt's laboratory he managed in his experiments on reaction time to do something in the way of reconciling the opposites: the conventional emphasis upon general lawfulness in science and his own very real concern with the problem of individual differences. Like Galton, Cattell turned to statistics and found in them a methodological tool for the solution of his problem. Of this problem and of Cattell's contribution to it, Boring has written, "The difference between individual psychology and general psychology can be regarded as a matter of statistical emphasis; the former centers attention upon deviations from the mean, the latter upon the mean. General psychology was so well established that the probable error needed a champion and it had one in Cattell" (1929, p. 522).

The reconciliation of the psychology of the individual with general psychology by way of the probable error is possible for psychologists like Cattell whose interests lie more in the degrees to which individuals differ from each other along continua common to all than in the ways in which each is unique. But it is not at all possible for those whose interests center not in the differences among persons but in persons *qua* persons. The former field of interest has quite properly been labeled as the psychology of individual differences, the latter as the psychology of personality. Yet it is clear that the two fields overlap and that many who work in one claim thereby to work also in the other or insist that in truth these are not two fields, but one. It is interesting to note, however, that these claims, although often made by the students of individual differences, are seldom made by the champions of personality.

The movement started by James McKeen Cattell was that of mental tests and measurements.[9] It has been a vigorous movement, contributing on the one hand to the development of an intricate and sophisticated statistical methodology for the measurement of individual differences, primarily differences in intelligence, but increasingly in other areas as well and, through the factorial analysis of matrices of intercorrelations of measured performances, contributing new conceptualizations of the structure of mind and personality (Guilford, 1936; Holzinger and Harman, 1941; Hotelling, 1933; Kelley, 1928, 1935; Thurstone, 1931, 1935, 1938, 1944, 1947, 1948; Tryon, 1935, 1939; Wolfle, 1940, 1942).[10]

The Factor Analytic Approach to Personality. It has been true in the past and it still remains true in large measure that students of personality fall roughtly into two classes those who believe in a richness and complexity of personality structure and those who

[9] The term "mental tests and measurements" was the title of an article published by Cattell in 1890

[10] For an extensive bibliography of factor analysis, both in the United States and in Great Britain, as well as for a general bibliography of attempts to describe and measure personality, Cattell, *Description and Measurement of Personality* (1946) is especially recommended. For critiques of the rationale of factor analysis and its applicability in the search for the structure of personality, see Tryon, 1935, 1939; Burks, 1936; Allport, 1937; Vernon, 1938; MacKinnon, 1944,

assert that personality has, at base, a simple structure and a relatively small number of traits or factors or dimensions. The clinicians, and most notably among them, the psychoanalysts, tend to favor the hypothesis of prodigality in the personality, while the experimentalists, and most notably among them, the factor analysts, are inclined to support the hypothesis of frugality of personality.

There are, of late, however, increasing signs of desertion from the camp of factor analysts who conceptualize a simple and parsimonious structure of personality. The history of factorial research in the cognitive domain bids fair to be repeated in the field of personality.

Spearman, the originator of factor analysis, proposed at first a single-factor theory of intelligence (1904). Both his theory and his methods were immediately criticized, and the controversy over them has raged without interruption to the present. Over this period, however, the trend in theoretical speculation, in modification of factorial methods, and in data actually collected, has been toward an increasing recognition of group factors. In the cognitive domain alone such group factors as the verbal, the numerical, and the visual have been demonstrated. More recently these have been shown to be capable of further analysis. Three verbal factors have already been discovered, and additional ones appear to be indicated. And what holds for the original verbal factor obtains also in the case of the other group factors which were originally delineated in the cognitive domain (Thurstone, 1948). If one may read the signs, it looks as if the notion that a relatively small number of factors would suffice for the adequate representation either of intelligence or of temperament or of personality, is by way of receiving the *coup de grace*.

The earlier, enthusiastic belief of factor analysts that their methods could be applied indiscriminately and without modification to any set of correlations is giving way to a much more critical attitude toward their application. Thurstone, expressing this view which sees factorial methods not only as something less but also as something more than they were early considered to be, reminds us that factor analysis "is not merely a statistical method, and it is not a

routine that can be applied fruitfully to every correlation table in sight" (1948, p. 402).

In recent years, the wisdom if not the necessity in any factorial study of starting with clearly formulated hypotheses based upon as much psychological insight as the investigator can muster has been increasingly recognized. Thurstone reports that in the Chicago Laboratory more time is spent "in designing the experimental tests for a factor study than on all the computational work, including the correlations, the factoring, and the analysis of the structure" (1948, p. 402). When such procedure becomes general practice, the contribution of factorial studies to the experimental testing of hypotheses in the field of personality should be greatly increased. And in the absence of such attention to theory, factor analysis can be of little value, for, as Thurstone has pointed out. "If we have no psychological ideas, we are not likely to discover anything interesting, because even if the factorial results are clear and clean, the interpretation must be as subjective as in any other scientific work" (1948, p. 402).[11]

The Experimental Approach to Personality in France and Germany.—The discovery of "the personal equation" and the undertaking to subject this phenomenon to laboratory investigation marked the beginning on the continent of the experimental approach to personality by way of the study of individual differences.

This interest led, on the one hand, as it also did in England and the United States, to the development of practical tests of ability and intelligence and, on the other hand, to considerable research into what may best be designated as an experimental typology of personality. Both of these interests began in the experimental laboratory but moved beyond it, in the former case most notably into the schools and in the latter case into psychiatric institutions. The former flourished especially in France, under the genius of Alfred Binet, the latter in Germany instigated by Kraepelin's early researches in the Leipzig laboratory

[11] The following studies may be cited as illustrative of recent factorial studies guided by considerable psychological theorizing concerning the source traits and structure of personality: Cattell (1946); Eysenck (1947).

Alfred Binet was one of the greatest of all French psychologists. His fame as creator, along with Simon, of the intelligence test has caused his earlier able work as an experimentalist to be neglected. With Beauvis he founded France's first psychological laboratory at the Sorbonne in 1889. It was, however, the problem of personality which came increasingly to interest Binet, and it was this interest which took him into the clinic where he studied suggestibility (1900) and even earlier into the schools where his investigations of the mind of the child eventuated in his experimental study of intelligence (1903) and the development of his famous test or scale for its measurement (Binet and Simon, 1905). The significance of Binet's work and its influence upon the subsequent development of the mental testing movement cannot be overestimated. From the standpoint, however, of our concern with the development of something approximating a full conceptualization of personality and of adequate methods for its investigation and measurement, the contributions of Binet, like those of most psychologists, have been to a single aspect of personality rather than to personality as a whole.

Clinical and Experimental Typologies.--The development in the modern period of the major attempts to depict differentiated or opposed types of personality was born of the union of psychiatry and psychology, a union which, however, was soon dissolved. The last twenty-five years have witnessed a second rapprochement between these two fields, but this time the fruitful rapprochement has occurred, not so much between psychology and psychiatry as between psychology and psychoanalysis.

In the early 1880's, Emil Kraepelin had worked in Wundt's laboratory on the problem of reaction time. When, a few years later, he turned his energies, as a young psychiatrist, to the task of introducing order into the nosological confusions in his field, he undertook an extensive program of research into the simple sensory and sensori-motor responses of patients in the belief that demonstrated differences in such processes might constitute a firm psychological basis for a more accurate description of and therefore a more adequate classification of psychiatric disorders. This work contributed to Kraepelin's establishment of the dichotomy of the two major

functional psychoses: manic-depression *vs.* dementia praecox, and to the attempt to discover those sensory, sensori-motor, perceptual and behavioral characteristics which would differentiate not only this dichotomy of psychotic types but an ever-increasing list of dichotomous typologies of personality, both normal and abnormal, proposed by other investigators. The history of dichotomous typologies, fed in part by clinical observation and in part by the experimental investigation of various psychological processes presumed to differentiate the types has been summarized and presented in tabular form elsewhere (MacKinnon, 1944, pp 16-25) and, therefore, will not be reviewed here.

The Psychoanalytic (Dynamic) Approach to Personality

The dynamic psychologies arose mostly as a by-product of the practical effort to treat the neuroses, especially hysteria. The Kraepelinian psychiatry, orthodox at the time, was static in the sense of being largely occupied with classification and diagnosis (nosology) and was of little practical help. Freud's new stress (1924) on the history, course and development of hysteria in the individual case, was in sharp contrast, for it meant inevitably a central concern with change, process and function rather than with structure and classification. This was also a reason, along with the wide difference in choice of problem and subject-matter, for the early separation of psychoanalysis from experimental psychology. For the practical purposes of understanding and curing patients, the Wundtian psychology was as useless as the Kraepelinian nosology.

More specific content was given to the concept "dynamic" by Freud's discovery of repression, of catharsis, and of frustration, by the assigning of a central role to the sexual "instinct," and later by the Adlerian discovery of compensatory reactions to inferiority. All these gave "dynamic" the core meaning which it has for most today, namely, the assignment of the basic role in all psychological theory to motivation, especially unconscious motivation.

Through all its schisms and revisions, psychoanalysis has remained steadily dynamic in these senses although the Freudian geneticism has not fared so well, as we shall see. It is this dynami-

cism which will probably turn out to be the main contribution of psychoanalysis, not only to personality theory, but also to general psychological theory. It seems more and more clear that any psychology which does not grant a leading role to motivation will not be able to survive.

Early dynamic theories of personality were, then, specifically designed and constructed for the immediate practical purposes of psychiatric treatment. Because of this closeness to the demands of practice, these theories not only affected treatment, but were themselves immediately judged, changed, or rejected, accordingly as they were or were not successful in improving treatment. Indeed, it may be argued that dynamic theories of personality, because of this closeness to the pragmatic test, have been subjected to external validating criteria to a far greater degree than have the other areas of psychology

Many of the faults and virtues of dynamic theories of personality are understandable only if this historical peculiarity is clearly recognized. Since almost the sole demand upon them by the clinician is that they work, and since such empirically workable theories are used even if the "professors," or the theorists, disapprove, it becomes understandable that the theories of Freud (1924), Adler (1917 a and b, 1929), Jung (1917, 1924, 1928, 1939, 1947), *et al.*, should *not* be marked by semantic accuracy, mathematical elegance, experimental-operational precision, philosophical stability or logical structure. Indeed, it is possible for a cure to be recognized even if it has no theoretical foundations at all, e.g , shock therapy.

Another consequence of this closeness to practice is that problem-centering becomes more and more important than method-or-technique-centering. Ordinarily, theory in psychology grows cautiously or conservatively, as a blind man walks Every step is tested, only safe moves are made, and a new position is taken up only grudgingly. Theory of learning, of perception, of thinking, of the determinants of behavior, are all examples of slow and safe theory construction.

Dynamic personality theory is often in a different position; it often happens that it is brusquely presented with a new problem which is not of its own choosing, which does not arise naturally and

spontaneously out of its own inner growth processes, but which is dictated by external forces. Typical examples are (a) Freud (1933) confronted with the puzzling dreams of the traumatized soldier; (b) American personologists, the OSS assessment staff (1948), confronted with the task of selecting intelligence agents; (c) American psychiatry faced with the unsought-for task of screening draftees; (d) psychoanalysts called upon to supply a theoretical rationale to the "psychosomatic" movement in medicine; (e) David Levy, *et al.* (1947), who had probably never before even *seen* a Nazi, suddenly given the task of separating Nazis from non-Nazis in postwar Germany.

In summary, then, dynamic theories of personality, because of their pragmatic orientation, tend to have less of the academic-experimental virtues (system, precision, certainty, logicalness, structure, orderliness). As compensation for this, however, they also have less of the shortcomings of the academic point of view (mustiness, obsessive-compulsiveness, removal from the concerns of ordinary human beings, lack of interest in humanistic and social validation). In short, the dynamic theories tend to be less methodologically elegant and more humanistically useful.

Facts and Theories in Freud.—In Freud (1924) we find the clinician and the theorist thoroughly intertwined. But he was, in approximately the first third of his productive life, the *practical* theorist, guided primarily by the necessity of explaining his psychiatric observations. For such a man theory is close to the facts, is always guided and determined by them, and is never very far ahead of them. It was only later on, roughly in the second half of his life, that he became more the speculative theorist, the metapsychologist rather than the psychoanalyst. Kimball Young (1940) has phrased it conveniently for our purposes by speaking of Freud I and Freud II as if they were different people.

Like any dichotomy, this one is simultaneously inaccurate and useful. It is inaccurate because Freud was a little metapsychological in his earlier years, and was still an empiricist and clinician in his latter years, and in any case, there was no sharp dividing line, or single year, or particular publication to which we can point as sepa-

rating Freud I from Freud II. *Totem and Taboo,* for instance, was published in 1918.

But it is also useful for the simple reason that most of what can be retained by the student of the *science* of psychology from out of all of Freud's theories and facts was written by Freud I. Contrariwise, most of what must be rejected for "empirical" reasons, because it does not stand up to detailed criticism, was written by Freud II. Who in psychology fails to read, for instance, the *Interpretation of Dreams* (1923)? And who in psychology bothers to read *Moses and Monotheism* (1939)? And what, then, by the way, can it mean to be "pro-Freudian" or "anti-Freudian"?

Originally offered as theories, but now so amply supported as to take on the status of facts in the area of personality, are (a) the fact of unconscious motivation, (b) the fact of repression, (c) the fact of the expressive and functional character of dreams, (d) the efficacy of free association, (e) the facts of defensive, "Freudian" and dream mechanisms, e.g., projection, identification, regression, reaction formation, rationalization, condensation, secondary elaboration, etc., (f) the facts of infantile sexuality, and of unavoidable intrusion of sexual impulses into other seemingly non-sexual areas of life, (g) the focusing upon frustration of deep-lying impulses as the arch-villain in psychopathology, (h) the discovery of the therapeutic efficacy of cathartic expression, (i) insight into the relevance of life history to the study of personality, (j) the (now reinterpreted) fact of transference, (k) the fact of resistance, (l) the frequent meaningfulness of accidents, slips, forgetting, jokes, etc. As a matter of fact, it may on the whole be fairly said that Freud as a clinical observer and fact-finder has stood the test of critical checking through the years. His *facts* seem to be beyond cavil; it is his speculations and attempted explanations of the facts that have either been rejected or unused by "scientific" students of personality. For instance, we may contrast the fate of his observations on child and adult sexuality with the very different fate of the libido theory; or the facts of self-destruction with his theory of the Death Instinct; or the facts about unconscious elements in family relationships with the theory of the Oedipus Complex.

Because of their special importance for both practice and theory, we must accord a little special attention to the theories of psychopathogenesis and of psychotherapy. These are still among the main sources of information and instruction for the student of personality. To take some recent examples, see Horney (1937), Fromm (1941, 1947), Alexander and French (1946), Axline (1947), Alexander (1948), and Porter (1950).

Psychopathogenesis.—All dynamic theories of the forces which give rise to psychopathology are variants of the Freudian scheme: (1) There are unavoidable "given" instincts or drives or needs or tendencies which demand satisfaction. (2) If these organismic demands are frustrated or if their gratification produces too much conflict, guilt, or anxiety, psychopathology may result.

There is considerable difference of opinion—and as yet very few unmistakable facts—about what these demanding impulses may be Psychoanalytic writers have stressed variously gregariousness, safety, love, power, status, ego-enhancement, protection, survival, etc. As a group, however, they have dodged the necessary theoretical task of producing clearly formulated theories which in turn can be validated or rejected by research.

Furthermore, the nature of these needs has not yet been discovered. Are they, as most practitioners assume, in some sense instinct-like? Or are they, as most psychologists assume, without further ado, learned or in some other way acquired?

Conflict and frustration have received somewhat more theoretical and experimental attention but remain barely touched in comparison with what their intrinsic theoretical importance demands. The provocative work of Maier (1949), Rosenzweig (1946) and their students are examples of the potential fruitfulness of research in this field.

The major accompaniments of consequences of conflict and frustration, e.g., guilt and shame, depression, anxiety, hostility, etc., all await mass experimental attack, for although there is a large literature on these topics, it is very largely confined to the psychoanalytical journals and generally has a doctrinaire rather than scientific flavor. That these subjects are even now susceptible to sound and fruitful

study is evidenced by various excellent studies of prejudice and of the prejudiced personality, e.g., Adorno, Frenkel-Brunswik, Levinson and Sanford (1950).

Psychotherapy.--It is amazing that psychologists have not turned to the study of psychotherapy as to an unworked gold mine. As a result of successful psychotherapy, people perceive differently, think differently, learn differently. Their motives change, as do their emotions. Their interpersonal relations and attitudes toward society are transformed. Their characters (or personalities) change both superficially and profoundly. There is even some evidence that their appearance changes, that "physical" health is improved, etc. In some cases, even the IQ goes up (Axline, 1949). And yet, the word is not even listed in the index of most books on learning, perception, thinking, motivation, socal psychology, physiological psychology, etc. Perhaps now that Rogers (1942) and his colleagues, and the group therapists, Coffey, Freedman, Leary, and Ossorio (1950), have demonstrated that experimentation in therapy is possible, the situation will improve.

Without attempting to assign individual credit for these various discoveries, we may say that psychotherapy takes place in six main ways: (a) by expression (act-completion, release, catharsis) as exemplified in Levy's release therapy (1939); (b) by basic need-gratification (giving support, reassurance, protection, love, respect); (c) by removing threat (protection, good sound political and economic conditions); (d) by improved insight, knowledge and understanding; (e) by suggestion or authority, and (f) by positive self-actualization, individuation or "growth." It is probable that all "systems" of psychotherapy use all of these basic "medicines" in varying proportions. For the more general purposes of personality theory, this also constitutes a list of the ways in which personality changes in culturally and psychiatrically approved directions. But since it is clear that such therapeutic effects are also exerted by good friendships, marriage, parent-child relationships, etc., these, too, constitute pressing and promising research problems. To take a single example, there is no question whatsoever that the theory of learning would, to say the least, profit by the study of the "learning"

effects of such therapeutic forces as marriage, friendship, free-association, resistance-analysis, success in a job, not to mention tragedy, trauma, conflict, and suffering.

Another fascinating set of unsolved problems is turned up by examining the psychotherapeutic relationships as simply a sub-example of social or interpersonal relationships, i.e., as a branch of social psychology. We can now describe at least three ways in which patients and therapists can relate to each other, the authoritarian, the democratic, and the laissez-faire, each having its special usefulness at various times. But precisely these three types of relationships are found in the social atmosphere of boys' clubs (Lewin, Lippitt, and White, 1939), in styles of hypnosis, in types of political theory, in mother-child relationships, and in kinds of social organization found in infra-human primates (Maslow, 1940)

Probably the weakest portion of the literature on psychotherapy is the delineation of the aims and goals of therapy Any thoroughgoing effort in this direction exposes very quickly the inadequate development of current personality theory, calls into question the basic scientific orthodoxy that values have no place in science, lays bare the limitations of medical notions of health, disease, therapy and cure, and finally reveals clearly that our culture lacks a usable value system. No wonder that people are afraid of the problem (But see below pp. 645 f on Growth and Self-Actualization)

Holism and Atomism in Psychoanalysis. –Of great theoretical importance, and ultimately of practical importance as well, is the correction of the ever-strong tendencies to reductionism and atomism in psychoanalytic theory Since so much of personality theory originated in psychoanalysis, correcting the one corrects the other.

Freud rested squarely on Nineteenth Century scientific theory in his reductionism, his tendency to analyze, to dissect, to dichotomize ("Aristotelianism"). Presented with a problem, Freud spontaneously and automatically split the field into two or three discrete parts, mutually exclusive, not at all alike in any way, and usually in antagonistic relations to each other, e g., the conscious *vs.* the unconscious, ego *vs.* id *vs.* superego, instinct *vs.* society, death instinct *vs.* life instinct. The individual's interests are for Freud intrinsically op-

posed to the interests of any and all other individuals. Even within the id, each impulse goes its own way, seeking only its own satisfaction, at whatever cost to other id-impulses or to the whole individual. For that matter, even the therapist-patient relationship is often discussed by Freud and many of his followers as if it were a sort of quiet, covert battle. Beyond a doubt, however, confusion has been sown most widely and most viciously by the Freudian assumption that the parent-child relationship is essentially one of antagonistic self-seeking and forced, *quid pro quo* collaboration rather than one of love-identification.

The chief contrast between the Freudian and Adlerian psychologies was probably in this basic theoretical prejudice, and it will be profitable for the student of personality theory to explore a few of the ultimate consequences of the difference.

For Adler, psychopathogenesis (to oversimplify) was in large part a consequence of breaking the patient's intrinsic identification with other people. Neurotic illness could be seen, then, as a fragmentation and breaking up of the intrinsic wholeness and internal collaboration of the individual. Psychotherapy must obviously be, at least in part, a restoring of the ties to others and of the synergic ties within the person. This could best be done in a big-brotherly and friendly therapeutic atmosphere (Adler, 1930, 1931; Crookshank, 1932; Mairet, 1928). The difference in conception of the parent-child relationship is just as striking and is in the expected direction (Adler, 1929; Dreikurs, 1948). It was specifically to stress this difference in basic theoretical outlook that Adler rejected the name "Psychoanalysis" in favor of "Individual Psychology."

This revision in the holistic direction has been carried forward, perhaps unwittingly, by such revisionist psychoanalysts as Horney (1939), Fromm (1941), and Sullivan (1945), but its fullest development came from psychology rather than medicine and will be briefly discussed below.

Environment and Culture; Over-Biologism; Over-Geneticism.—The main development in the history of psychoanalytic theory of personality has been the growing appreciation of cultural and situational forces. The earliest writers on this subject in English

were Horney (1937), and Fromm (1941), and more recently Sullivan (1945), Kardiner (1939, 1945), and Erikson (1945, 1946).

One by one the main tenets of orthodox Freudian theory were corrected, revised, or discarded as made necessary by the application of culture theory, e.g., theory of anxiety, Oedipus Complex, libido theory, instinct theory in general, penis envy, the role of the female, guilt, superego and much else as is made quite clear in Thompson's excellent summary (1950). These writers have certainly established that personality both in current functioning and in its formation and course, *must* be related to culture and environmental determiners. With this emphasis most recent treatises on personality agree (Murphy, 1947; Stagner, 1937; Harsh and Schrickel, 1950). It is not an accident that Personality and Social Psychology form a *single* division of the American Psychological Association. The Freudian stress on powerful bad-animal-instincts, autonomously working themselves out in almost complete disregard of culture has been completely discarded by all psychologists, although not by all psychoanalysts (Fenichel, 1945; Roheim, 1943).

But now that this culture-personality interdependence has been established, the criticism must be made that especially in the writings of Fromm, the personality, in the biological, constitutional sense, is in some danger of evaporating away entirely into a "something" (what?) that is produced and shaped almost alone by extra-organismic forces.

Kardiner (1939, 1945) attempts to interrelate intra- and extra-organismic forces so as to avoid this criticism. Instinct-theory in his writings has been corrected and improved but not completely discarded. For him, culture impinges on an organism that has a biological "nature" of its own, antecedent to culture The same may be said of Levy (1947). Perhaps from such an intermediate orientation may yet come the consideration of certain theoretical problems which have been neglected in the "rush to culture," e.g., resistance to acculturation, autonomy of the personality, criticism of the culture from within, the concept of spontaneity, the concept of self-actualization, the criticism of hedonism, supra-cultural and

cross-cultural values, species-characteristics, the role of reality, of knowledge and of truth in the development of personality.

Holistic Theories of Personality

The Goldsteinian (1939, 1940, 1947) holistic (or better, organismic) theory of psychology and of personality developed in contact with the Berlin school of Gestalt psychology, and for a time, in collaboration with it. Its full impact has yet to be felt, for it is so radical a deviation from the other main sources of theory that it may take another decade for psychologists to digest it. Angyal's important book (1941) is the first step in this direction

Goldstein's central observation was that organisms which are severely damaged are *not* simply the same as they were before, minus whatever they lost Rather, an organism damaged at its core will spontaneously reorganize itself into another kind of whole organism which retains all its former functions, aims and hopes, but at perhaps a less efficient or "lower" level. It does the best it can, under the new circumstances. In a word, all abilities are seen by Goldstein as of the whole organism, rather than as isolated parts of it. For him, the organism definitely has a "nature" of its own, in accordance with which it selects stimuli and behaves. That is to say, one cannot understand either the nature of stimuli or the nature of behavior except as functions of the nature of the particular organism. An ultimately similar point of view is expressed in Werner (1940) and similar difficulties of comprehension and assimilation with other points of view are encountered.

The main criticism that can be leveled against the holistic theories is that they have not made use enough of the discoveries of the psychoanalysts. A holistic-dynamic fusion of psychoanalysis and Gestalt psychology has been attempted by Maslow (1943, a & b) and by W. Wolff (1947).

Self-Actualization and Growth.- Goldstein's dynamic principle is the trend to self-actualization, the fullest realization in actuality of the potentialities and intrinsic nature of the organism (Angyal 1941, Maslow 1950, Rogers 1948, and Fromm 1947). All other

motives are seen as only partial steps in this direction rather than as separate and independent goals.

Most important for motivation- and value-theory is the introduction of a positive force to supplement the Freudian pessimism and the neo-behavioristic relativism which in general assumes that the organism is ultimately motivated by avoidance of punishment, the relief of tension, and the seeking for a few physiological pleasures, e.g., food, sex, etc., and by whatever can be learned on this basis.

The intrinsic difficulties and shortcomings of this position in explaining such complex human activities as science, philosophy and art, not to mention pathology and therapy, have, in general, been avoided by studying mainly crippled people and desperate rats. That is to say, the problem has simply been side-stepped. To Jung (1939) must go the credit for first perceiving and criticizing this shortcoming in Freud. He has always placed great stress on the urge to healthy and full development of the patient. However, his concept of "individuation" is so vaguely described that it has been very little used by American psychologists.

What such a positive concept can do for psychology is seen in the numerous writings of Rogers (1942, 1948) and his students, in which the concept of "growth" (indistinguishable from self-actualization) assumes more and more a central and essential role. This can be equally so for a psychological theory of democracy, of interpersonal relations, of social improvement, of cross-cultural comparison, and of a scientific system of values. With its aid there is no reason why cognition, conation, and affection should not be tied together once again, i.e., the contrast between individual and social, between selfish and altruistic, between instinctive and rational, and many other such false dichotomies can be resolved.

General-Semanticism

A growing influence in psychology has been the "general-semantics" movement fathered by Korzybski (1933) and interpreted by Johnson (1946) and Hayakawa (1949). It is not alone the nature of language and communication which they expound, but also its influence on the perception of reality, on interpersonal relations, on

psychopathology and on psychotherapy. It is based squarely on the radical acceptance of the holistic fact that the world is a unity in the sense that all its parts are related to each other, one way or another, and on the ultimate dynamic fact that all things change and grow.

The implications for personality theory are considerable. For instance, it is Maslow's (1948) claim that the average "normal" person who thinks he is living in the real world and among real people is far more likely to be reacting to stereotypes, abstractions, and "rubrics" in his own head. The best known example is prejudice, but so many other examples of cognitive distortion can be given in so many other areas that a serious shadow of doubt is thrown on the validity of the assumption that the normal person is determined primarily by the real world. Sullivan's (1945) concept of "parataxic distortion," an improvement upon and generalization of Freud's "transference" theory, is a psychiatric version of the same point.

Summary

The field of personality is extraordinarly rich in fascinating hunches, hypotheses, and theories. These have come in greatest number and with greatest richness from psychiatrists and clinicians, who, however, have not been noted as a group for experimental emphases or methodological sophistication. Fortunately, there are mounting signs of an integration of personality theory into the general theory of psychology. To the extent that a true rapprochement is achieved, psychology will increasingly focus its attention upon the more dynamic and more molar aspects of behavior, and, in turn, the various and often conflicting theories of personality development, structure, and function will be subjected to the test of rigorous scientific investigation which alone can yield the facts required for their ultimate rejection, modification, or validation

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CHAPTER 14

PSYCHOLOGICAL THEORY AND SOCIAL PSYCHOLOGY

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It is extremely difficult and always foolhardy to attempt to evaluate the contemporary scene from an historical perspective. Nevertheless it is perhaps safe to venture the prediction that the current period, roughly the middle decades of the twentieth century, will be recognized as a significant turning point in the application of psychological theory to the data of social psychology, and the use of social psychological data in the development of psychological theory. However true this may turn out to be, the *past* relationship between social psychology and psychological theory reminds one forcibly of the old adage that it is the children of the shoemaker who go barefoot.

The psychologist has never been known to be bashful about propounding and adumbrating theories, yet *social* psychology, in the past, has suffered from extreme and extended poverty in theory—it has been the neglected waif of the theoretical-minded psychologist. Social psychology in the hands of the sociologist or social philosopher never lacked for theoretical orientation -the orientation, naturally, of the sociologist or social philosopher. But once the experimenting and objectively-minded psychologist claimed jurisdiction over the field, social psychology remained for a very long time in a theoretical void and without conceptual form In part this situation was due to the beliefs and attitudes of the social psychologist, and in part it was due to the scientific snobbishness of the theoretical-minded psychologist.

About the time that the field of social psychology was being admitted to departments of psychology, most psychologists were very busy being over-impressed with the empirical approach and

with the importance of "objective facts"--theory was to be eschewed. This attitude was reflected in the work of the new social psychologists. Their task was to weigh and measure the objectively observable behavior of man in society. Theoretical speculation could wait. This attitude was strengthened by their judgments that the early non-psychologists who had worked in the field, Le Bon and Tarde, had over-theorized, under-observed, and had altogether built a house of fantasy in social psychology. And as for such psychologists as McDougall who had concerned themselves with social psychology—their theories were now regarded as quaint theologies and were held in complete disrepute. In over-reaction to such over-theorizing (and, in their view, bad theorizing), the early experimental and social psychologists showed a complete lack of interest in developing a well-rounded theoretical structure for their field

The lack of development of theory was not, however, altogether the doings of the social psychologist. The theoreticians among the psychologists must also share part of the blame. To them social psychology was completely without scientific status. The social psychologist worked with ephemeral and practical problems rather than with the eternal verities of pure science. The actual work of the social psychologist, furthermore, was of such a nature as not to permit of the nicely controlled and simplified experimental situation or even, in many cases, of the possibility of repeated observations. Finally, the social psychologist, as an individual, was frequently regarded as being profoundly uninterested in, and incapable of appreciating, theoretically important issues The whole field was judged, by the theoretician, to be somewhat raw, disquietingly unorthodox, basically unscientific and with a slight flavor of ill-repute about it. Whenever the theoretician did use social phenomena to illustrate his theoretical concepts, he did so in a left-handed, almost abashed way as if he were merely being "cute" or else on a scientific slumming junket and hoped that his colleagues would not take him seriously.

To a significant degree the above over-simplified description of the attitudes of the social psychologists and the theoreticians no longer applies. Today, and this is the major burden of this chapter,

some of the most important theoretical developments in psychology are occurring within the field of social psychology and through the work of social psychologists. However, the above account, unfortunately, has more than an historical interest. In at least two major fields of social psychology there still remain some practitioners who are direct spiritual descendants of the early anti-theoretical social psychologists, or who still fail to understand the role that basic psychological theory can play in the various specific "fields" of psychology.

Before beginning the discussion of current theoretical developments in social psychology it may be desirable therefore to review briefly the work and status of those social psychologists who are only peripherally interested in theory—the relicts, as it were, of the social psychologists described earlier. Such a review should permit us to evaluate better the role of theory in social psychology.

Today's A-Theoretical Psychologists

There are, as has already been indicated, two groups of social psychologists who show a cultural lag as far as attention to theory is involved. The first of these two groups, best exemplified by the psychologists in the field of attitude testing and public opinion measurement, might be called a-theoretical social psychologists. The second group, found primarily among many of the textbook writers in social psychology, seem to hold the view that theory is a necessary evil—but not a very helpful evil. We will briefly consider each of these in turn.

The Opinioneers—Social Psychology without Psychological Theory.—Perhaps the most active field in social psychology, the field with the most journals specifically devoted to its doings, the field which receives the most financial support, the field which represents social psychology to most laymen, is the field of public opinion research. And perhaps the most thoroughly tilled field in academic social psychology, the field which has yielded the most bountiful crop of M.A.'s and Ph.D.'s in social psychology and which has resulted in the most publications among the general journals of social

psychology, is the field of belief and attitude measurement. And yet it is these two fields which, until very recently, have shown an extraordinary lack of concern with fundamental psychological theory. This has been especially true of the first of these fields.

Basic psychological theory and concepts seem to play no role at all in the thinking of public opinon pollsters. Although these social psychologists attempt to measure beliefs, attitudes, judgments, and opinions, neither the commercial polling organizations nor the academic survey research groups have shown any significant tendency to concern themselves with the *psychological theory* of beliefs, attitudes, judgments, and opinions. The public opinion journals are filled with well-designed and statistically sophisticated studies which attack such problems as question-construction, interviewing techniques, sampling procedures, coding operations, etc., but it is only rarely (if at all) that one can find in these journals any mature discussion of the basic nature of the "things" which these questions, interviews, and codes are presumably measuring, weighing and reporting. One can find a concern with the theory of *techniques* sampling theory, measurement theory, interviewing theory, etc.- and much of this theory represents some of the best and most ingenious thinking in psychological literature, but there is no concern with the theory of *that which is being measured.* This is a particularly serious lack because the concepts of beliefs and attitudes are among the most fundamental of all concepts in psychology.

A very brief discussion of but one consequence of this lack of theoretical concern may serve to emphasize its dangers.

Measuring What Isn't There.[1]---It may seem so obvious as not to merit mention that no person's opinion can be measured unless he has an opinion. Yet an examination of much of the work in belief and attitude testing leads one to believe that this obvious fact has not been recognized. So much of the work seems to be based on the assumption that all one needs to have is some sort of measuring instrument and that one can then apply this measuring instrument to any group of people and proceed to measure beliefs and attitudes

[1] The following discussion is taken from an earlier article by the present author. See Krech, D. (1948).

among people who do not have an opinion but who *should* have. This strange position reflects the naïve "operational" notion—sometimes explicitly and sometimes implicitly stated—that an attitude or an opinion is that which an attitude test measures. There is a complete failure to be clear as to the difference between a verbal *reaction* and a *belief*.

Whatever else a theoretically sound definition of a belief or attitude will involve, it seems clear from our present knowledge that these terms refer to some sort of *enduring* cognitive or dynamic structures which function as *intervening variables* in the total economy of behavior. Beliefs or attitudes cannot be identified with ephemeral reactions or casual responses to an interviewer's questions. Certainly this is true, on the very face of it, for those beliefs and attitudes which make any significant difference in the behavior or personality of the individual. As many writers have frequently pointed out, people can respond in some way to *any* test item, but to identify such a response with an enduring structure or dynamic system is to be guilty of a very crude error. No instructor in psychology, for instance, has ever assumed that a student's response to any one true-false question is a necessary indication of an enduring bit of knowledge about the matter covered in the question—a knowledge which will affect his behavior in any degree.

No one has ever attempted to make a complete inventory of all of a single individual's enduring dynamic systems (beliefs and attitudes). Merely to contemplate such a task would be enough to convince one of the impressive number of man's beliefs and attitudes. *But the number of any one person's beliefs and attitudes is finite.* He can have beliefs and attitudes only with respect to those objects which exist for him in his own idiosyncratic psychological world. We cannot assume that the members of the same culture will all have beliefs or attitudes about a "common" set of issues. From what we already know about the operation of the basic psychological processes involved in belief and attitude formation and the processes of perception, we know that exposure to a common range of stimulation will not result in the growth of a uniform set of beliefs. An individual's enduring dynamic systems develop in terms of his *own* perceptions, not the perceptions of the sociologist who views

his culture. And perception is *functionally selective.* Therefore the development of any enduring dynamic system is intimately related to the needs, aspirations, and frustrations of the specific person. Not every labor union member, for example, has beliefs and attitudes about the Taft-Hartley Act or the Marshall Plan, no matter how urgently the pollster may wish to measure and report upon such beliefs and attitudes. Not every college sophomore has beliefs and attitudes about the church, or politics, or fascism—no matter how desperately the Ph.D candidate wishes to apply his laboriously constructed and statistically refined "attitude test" to his subjects. It will do no good to reply that those who have no enduring dynamic systems with respect to these issues can always say "Don't know." To repeat: Almost anyone can, if urged, react with a momentary "approval" or "disapproval"—even on a five-point scale—to any question relating to any matter

The failure of so many of our attitude testers to worry out a theoretically clear distinction between a response and an enduring dynamic system, a reaction and an intervening variable, has resulted in a tremendous amount of busy-work, of measuring and adding and percentaging ephemeral reactions of people who should have opinions but who don't. In a word, they have been busy measuring they know not what Illustrations such as the above can be multiplied. The moral seems clear Whatever the specific interest of the social psychologist, and no matter how "practical" his purpose, he cannot do good work without good theory

Social Psychology Textbooks—Theory Divorced from Reality. -If the fields of attitude testing are the representatives of social psychology to the public, the textbooks in social psychology are the representatives of social psychology to the students. And many of the textbooks in the field, although not neglecting psychological theory completely, have made very little use of it Many of our textbooks in social psychology can be seen as divided into two separate, independent parts where neither part is on speaking terms with the other. The first part usually consists of an abridged treatise of the principles covered in a course in introductory psychology, and in that part a few genuflections

are made to psychological theory. In the second part the writer concerns himself with a discussion of the "facts" of social psychology which consists primarily of a review of many of the experiments and studies specifically within the field of social psychology. But no consistent and systematic attempt is made to use the first part, the theoretical part, in analyzing, or criticizing, or understanding the material of the second part. Each social psychological study is presented as a thing complete within itself. As one eminent psychological editor has remarked, the textbooks in social psychology are merely glorified *Psychological Bulletin* reviews. Many studies are mentioned, many are abstracted, and the immediate practical implications of some of the studies are drawn out, but no study is examined for its fundamental theoretical significance or meaning, nor is any one theoretical treatment, based upon the first section of the book, applied systematically to these studies. Binding together within one set of hard covers a brief statement of psychological principles together with an account of social psychological studies is a far cry indeed from writing a theoretically oriented and mature social psychology.

This criticism has implications beyond the writing of textbooks. The same kind of criticism can be applied to some of the experimental workers in social psychology and especially to the student who is eager to get out into the field and get his hands on some *concrete data*. Before any one can say that he has applied a theoretical structure to the data of social psychology he must not only be aware of theory but he must take the pains to *spell out* the operation of his theoretical principles in the context of his social psychological data. Nothing else will do, nothing else will permit him to gain the wealth of insight there is to be gained from theory in analyzing data, and nothing else will enable him to assess the adequacy of his theory. The separation of the concrete data and *specific* experimental hypotheses of a specialized area of a science from the *general* principles of the science can be fatal for both theory and experiment. The theory can become esoteric, and the experimental facts sterile. Every theoretical principle should shed additional light on the significance of a fact, every fact should help support or modify a theoretical principle.

It is probably not far from the truth to say that most of the students in social psychology, in so far as they have depended upon our textbooks for their orientation and training in social psychology, have been exposed to both theory and data, but their theory was divorced from data, and their data from theory.

However, as has already been indicated, the status of theory in social psychology is not adequately represented by the above two areas of social psychological activities. No field of science shows a smooth and even front of change. There always exist differential rates of development in the different subfields of any science, and some of the subfields may lag behind others. Recent work of social psychologists, of laboratory workers in the fields of perception and learning, of clinical workers, and of many other psychologists has resulted in highly promising and exciting developments in psychological theory—theory which is being directly applied to many of the traditional problems which have been the concern of the experimental social psychologist. It is these developments to which we now turn.

Two Major Theoretical Issues

There have been two major and closely interrelated questions that have consistently plagued every theoretician in social psychology. One of these is the relation of social psychology to general psychology. The question here is whether the principles which the general psychologist discovers (or invents) in his laboratory are also the principles which the social psychologist will find applicable to his own data In other words, are there two psychologies—general and social—or only one? The second issue is that of the unit of analysis. As most commonly stated this problem resolves itself down to the question of whether we should use the *individual* or the *group* as our unit of analysis of social behavior.

At first examination it would appear that if we answer the first question by affirming an identity between general psychology and social psychology then it would necessarily follow that we would have to use the individual as our unit of analysis—for the behavior of the individual has been the traditional concern of the laboratory

psychologist. Or, by the same token, if we decide that the proper unit of analysis for the social psychologist is the group, then we cannot identify social psychological principles with general psychological principles. However, further consideration would seem to suggest that this choice, as is true of all "either-or" choices, is not a real one. Recent developments in both social psychology and general psychology indicate that the above dichotomy between the individual as a unit of analysis and the group as a unit of analysis is a questionable one, and that whatever unit of analysis we may prefer to use (and one can use *several*, depending upon the nature of the problem) general psychology, as a basic science, *must be identified with social psychology at every point*

General Psychology Is Social Psychology.—Perhaps one of the earliest statements of what is rapidly becoming to be recognized as the most tenable position on the "Individual *vs* Group" controversy, is that of Cooley (1902) who pointed out many years ago that,

> ". . . just as there is no society or group that is not a collective view of persons, so there is no individual who may not be regarded as a particular view of social groups He has no separate existence, through both hereditary and social factors in his life a man is bound into the whole of which he is a member, and to consider him apart from it is quite as artificial as to consider society apart from individuals" (Cooley, 1902, p 38)

Cooley's statement suggests that, not only are the social psychologists in error who insist that the only unit we need consider is the group, but that the general psychologist is also in error when he believes that the only unit he need consider is the individual. It is the recognition of this latter error which has been slowly borne home to the laboratory psychologist and which is now resulting in some significant re examinations of the most cherished and "immutable" laws and principles which have ever come out of the psychologist's laboratory. And this recognition has been borne home to the experimental psychologist in the most effective way possible—not through programmatic statements, or generalized statements of beliefs, but through well-designed and ingenious experimental analy-

ses of laboratory-born principles. It is here that the experimental social psychologist has made his greatest theoretical contributions.

The Theme of Current Experimental Work.—The major theme of the experimental work of the social psychologist which has forced a re-examination of the principles of general psychology can be stated very simply. Every basic psychological process of man—his perception, learning, remembering, wanting, feeling, etc.—must be understood within the social context of the perceiver, the learner, the rememberer, the wanting man. The most recent formulation of this principle was stated by Crutchfield and the present writer (Krech and Crutchfield, 1948, p. 8) as follows:

> ". . . the general psychologist as well as the social psychologist is literally forced to study the behavior of man as a social being. Whether we are studying the behavior of a man in a laboratory, in the clinic, or in a crowd, whether we are studying his perception of colored papers, his performance on an intelligence test, or his decision about participating in a lynching, we are studying the behavior of a man as influenced by his perception of the social world. . . . *The effects of a man's group membership, of his experience with other men, of his past and present interpersonal relationships reach into each of his psychological activities no matter how simple or apparently remote* . . . no psychologist, whatever his interests, does or can study the behavior of an asocial man."[2]

This position, of course, presents a serious challenge to the experimental psychologist. For the experimental psychologist has always assumed, usually implicitly, that in studying perception, for example, he was studying a process independent of the social background, affiliations, prejudices, and values of the perceiver. It didn't matter to him whether his subject was a student at Berlin University or at Swarthmore College; whether his subject came from the poorer lower class or the wealthier middle class. Among the variables which the perception psychologist, or the learning psychologist, included in his theoretical model of the perceiver and learner there was no room for social values and mores. In so far as he recognized

[2] Italics not in the original.

the influence of these factors, they were ascribed to interfering and uncontrolled influences—"imperfections" in his subjects. The psychologist here was taking the same way out of a difficulty that the classical economist takes when it is brought to his attention that "economic man" does not exist, that the motivational structure of man cannot be described in terms of "rational choices based only on money values." Such "interfering" motives as need for status, love, etc., are dismissed by the simon-pure economic classicist (in so far as such an economist exists outside of the elementary economics textbooks) as mere "imperfections" in the model. But, to return to our general psychologist. If the position outlined by Krech and Crutchfield is correct, then we cannot explain away those "imperfections" as nuisances which "mess up" a good experiment. We must realize that the social values and mores of man are just as significant a set of variables as any others, and that therefore many of the general psychologist's principles and laws must necessarily be in error—for he has omitted a whole family of important variables in his theoretical formulations.

One of the first carefully thought out and telling presentations of experimental data which launched the attack on the "asocial" general psychologist came with the publication of Bartlett's now classic book, *Remembering*, in 1932. Here, for the first time, was the principle generalized that while there appeared to be universal laws of memory and perception, the nature of these processes and the specific errors in memory and perception which people made could not be understood except in terms of the cultural backgrounds and socially determined interests and mores of the subjects. His book is replete with interesting experimental demonstrations of this effect.

With the publication of this book a whole series of experimental invasions of the learning and perception psychologist's laboratory by social psychologists was begun. Among these must be listed the well-known study of the autokinetic movement by Sherif (1935)—an experiment which he titled "A study of some social factors in perception," the last phrase of which became, for a while, a sort of a rallying-cry for many experimental social psychologists.

A concurrent experimental development which started off from a slightly different orientation but which is ending with the same general conclusions provides an interesting illustration of what happens when a science begins to hew to the line of the one cosmic principle of all sciences—*Everything depends upon everything else.* A number of psychologists, interested in perceptual problems, began to question the severance of personality and motivational factors (or "autistic" factors as Murphy preferred to call them) from purely perceptual factors Thus Thurstone (1944) pointed out, "In these days when we insist so frequently on the interdependence of all aspects of personality, it would be difficult to maintain that any of these functions, such as perception, is isolated from the rest of the dynamical system that constitutes the person" (p. 3), and Bruner and Goodman (1947) listed the programmatic implications of this position as follows: "What we must study . . . are the variations perception it on undergoes when one is hungry, in love, in pain, or solving a problem. These variations are as much a part of the psychology of perception as Korte's Laws" (p 33).

Among the experiments which attacked perception from these viewpoints the following pioneering studies must be cited: Sanford's (1936, 1937) studies of the effects of food deprivation upon perception and the "imaginal processes"; Proshansky's and Murphy's (1942) investigation of the effect of reward and punishment on perception; and Schafer and Murphy's studies (1943) of the visual figure-ground relationships—studies which seemed to indicate that what was figure and what was ground in a simple visual perception was influenced to a considerable degree by needs, rewards, and punishments. Finally, in 1947, with the publication of Bruner's and Goodman's study entitled "Value and Need as Organizing Factors in Perception" Bruner and his co-workers have initiated an ambitious and systematic re-examination of many of the "basic" principles of perception

Once it became clear that perception was no longer isolated from the needs and wants and experiences of man, the next step became obvious. Since needs, wants, and experiences are so intimately related to social factors, perceptual and learning laws could no longer be isolated from the *social milieu* of the perceiver and

learner. And so these studies also ended with the general conclusions which Bartlett saw so clearly. At the present writing the situation can be summarized by paraphrasing Thurstone's statement to this effect: "It would be difficult to maintain that any of these functions, such as perception, is isolated from man as a social being--from the effects of his group membership, interpersonal relations, etc."

We will return to a consideration of these new perceptual studies later, but the critical point to make here is that *the general psychologist has not and cannot formulate valid psychological principles which are "asocial." Out of his work and out of his thinking must come the principles which are immediately applicable to the work and thinking of the "social" psychologist.*

Some Additional Theoretical Considerations.--The identification of social psychology with general psychology does not rest solely upon the above considerations, i.e., that the general psychologist is *forced* to work with a social man and therefore he is *forced* to come out with "social" principles. Crutchfield and the present writer have suggested a purely theoretical consideration which supports the conclusion of identity. The following argument is adapted from their more extended discussion (1948, pp. 8-11).

A commonly accepted formulation of the difference between social psychology and general psychology is that the social psychologist studies the behavior of man in a social field, while the general psychologist does not. If we then ask what it is that uniquely characterizes a social field, the usual or common-sense answer is that it is the presence of other *persons* which differentiates a social from a nonsocial field. If that is so, we must next ascertain what especially significant characteristics these "person objects" have in governing the behavior of the individual. Why, in other words, should we give special emphasis to "person objects" among the many determinants of individual behavior. How does a "person object" differ from a "chair object," or "apple object" in affecting man's behavior?

Krech and Crutchfield then suggest, although admittedly without adequate experimental or observational data, the following theoretical considerations: Person objects differ from many other objects

because they are richly endowed with the following properties: *mobility, capriciousness, unpredictability, reciprocal sensitivity, loci of causality, power qualities* (i.e., they are perceived as being capable of providing rewards and threatening punishments). These characteristics, or qualities, or attributes of person-objects make them particularly significant stimulus objects in a person's field, and seem to set off a field replete with person objects from fields devoid of such person objects. But, and this is the crucial theoretical argument here, *these qualities which give person objects their great importance as stimuli are not unique to human beings.* Thus, for example, the lower animals also have these qualities, and even more significantly, even plants or inanimate objects may be perceived with these qualities. They offer the following illustrations:

> "Clouds and storms and winds are excellent examples of objects in the psychological field that carry the perceived properties of mobility, capriciousness, causation, power of threat and reward. So, too, the sea and the river and the ocean. So, too, the volcano, the glacier, the fire, and the earthquake. Thus a man living in an environment completely without animate objects might still exhibit some types of behavior that we ordinarily think of as social. He might punish a 'malicious' stone that trips him, try to appease thunder and lightning, imitate the noises of the waves, etc." (p. 10).

To argue that such perceptions are the fantastic perceptions of illogical children, savages, or of people exposed to superstitious "animistic" training, seems inadequate Certain perceptual qualities are immediately and directly inherent in the objects perceived. The perceptual experience of the capriciousness of a windstorm does not depend upon any "training" –it is immediately given, and so is the perception of capriciousness in a person object. No complicated logical or illogical reasoning process need be involved—any more so than there need be a complicated reasoning process in perceiving the form of a circle.

The critical point here is that much of what we commonly call "social behavior" gets its special qualities by virtue of the fact that the individual is being stimulated by capricious, mobile, etc., objects.

Since objects which have these qualities are not limited to "person objects" a social field cannot be identified with a peopled field. There is no sharp theoretical distinction between a peopled field and a field devoid of people; there can only be a *continuum*. This continuum ranges from simple fields (those which include only rather static, immobile objects) to complex fields (those which contain mobile, powerful, capricious, causal objects).

The above argument may seem to partake of the characteristics of a lily-gilding operation. Since there are sufficient *practical* grounds to indicate that there are very few psychological laws which can be written without including the so-called social variables, it would seem unnecessary to proceed (as we have just done) to prove that there is no *theoretically* meaningful distinction which can be drawn between a social and a nonsocial field Perhaps one of the values of the immediately preceding theoretical analysis (outside of the feeling of closure it may give the theoretician to prove, on purely theoretical grounds, what he already knows on empirical grounds) lies in the implied warning to the social psychologist who may resist paying attention to some of the pure research of the general psychologist. He might argue that there may be some instances in which "social" factors are *relatively* unimportant, and therefore we can go ahead without too much attention to certain of the theoretical issues raised by the general psychologist or to certain of his experimental concerns This neglect of some of the experimental psychologist's work, however, is unwarranted, since only in the very rare cases where the experimental psychologist has worked with limited responses of organisms to an environment devoid of any capricious, mobile, etc., objects can one assume such work to be unimportant for the social psychologist. It appears, to paraphrase Terence, that *nothing psychological should be foreign to the social psychologist.*

But whatever weight may be attached to this particular theoretical analysis as compared to the experimental data previously referred to, the general conclusion that as far as basic science is concerned there is but one psychology (not two: one social and one general) seems to be the soundest position in the light of available evidence.

Units of Analysis—A Functional Answer.—In terms of the second major problem, the unit of analysis, the above considerations seem to suggest that the choice which confronts us is not that between the *individual* and the *group*, but between the *individual-in-society* and the *group*. But even this reformulation is too rigid and does not correspond to the actual choices before us. The thesis can be defended that the proper unit of analysis is to be determined *functionally*, i.e., in terms of the specific problem which is under investigation at any one time.

The General Psychologist's unit of Analysis.—We must avoid making the common error that the unit of analysis used by the general psychologist is the "individual"—or, even the "individual-in-society." The general psychologist only very rarely uses the individual-in-society as his unit of analysis. His analysis is made in much more molecular terms. And from problem to problem he will use different units of analysis. Thus, in some instances, he will use the "habit" as his unit of analysis, and analyze man or behavior in terms of "habits." In other instances he will use "motives" or "percepts" as his analytical units. It should be clear, however, that no matter into how many fine pieces he may cut man, for purposes of analysis, each one of these pieces retains the significant characteristics of the individual-in-society. Thus, if the whole man is his unit of analysis, it is the "individual-in-society", if a "habit" is his unit, it is still a "habit of an individual-in-society"; and if a perceptual process is his unit, it is also "a perception of an individual-in-society." No unit can escape that characteristic and still remain a *psychologically* meaningful or useful unit. Once he has adopted a unit of analysis which no longer has that characteristic (as, for example, in such instances where the "reflex" is the unit of analysis) he no longer is asking a psychological question but a physiological, or chemical, or physical question.

It seems then that we can say at least three things about the unit of analysis as used by the general psychologist: (1) he uses *various* units, the specific nature of any unit being determined primarily by the nature of his question; (2) no matter which unit he uses, these units partake of the characteristics of the *social;* and (3) the only

limitation on the units which he can use is that the unit be *psychological* and not, say, physiological, or chemical, or physical.

The Social Psychologist's Unit of Analysis.—The first thing to which many theoreticians will agree is that the general psychologist's units of analysis (as defined above) are also available to the social psychologist. That is, the social psychologist may, for certain problems, use the perceptual processes as his units of analysis; or he may use habits of individuals as his units of analysis; or he may use beliefs and attitudes of people. The disagreements come, however, when it is suggested that the social psychologist may also use the *group* as his unit of analysis. It is to this problem which we must now turn.

There are at least three major sets of meanings which are currently given to the "group" as a unit of analysis. In the first of these the group is used merely as a statistical term for a defined and specified collection of people. In the second usage, the people making up the group are seen to stand in dynamic relationships to each other, and here the term is used as a short-cut device when one attempts to give an over-all description of what happens. In these two usages nothing is implied as to the superordinate nature of the group. Rather, the implication is that the study of groups can reveal nothing beyond what would be given by a synthesis of all the data pertaining to each member of the group. However, the third usage carries with it the definite implication that there are unique attributes of a group which can never be discovered by any synthesis of data obtained from the members of the group studied individually. A further analysis of each of these usages may make the distinctions clearer and may more accurately define the theoretical status of each.

The "Group" as a Unit of Analysis.—The first usage—the straightforward statistical one—can be illustrated by referring to F. H. Allport's studies on the conforming behavior of people in society. Allport's J-Curve hypothesis does permit one to study the conforming behavior of any functionally defined aggregate of people—automobile drivers, church-goers, factory employees, etc.,—and to write one's conclusions in terms of such a "group." That is, the

unit of analysis is an aggregate of people, and the resulting distribution curve of behavior may be said to characterize that particular aggregate. But it is obvious, from the very statistical methods used, that this theory asserts that the group is merely to be seen as a collection of discrete individuals with their own beliefs and behavior patterns who are alike in that they are all church-goers, say, but who differ with respect to certain specified aspects of behavior. The measure of variability among this statistical population can be seen as a "group characteristic."

Many of our public opinion studies, to take another example, also use the term "group" in that sense. What do Republicans think about the United Nations? What do Democrats think about the same problem? The answers to this question can be stated in group terms, and help to characterize the two groups—Republicans and Democrats, but in a statistical sense only. Another illustration: the term "national character" has certain value as a statistical concept, and can be used, if we understand that it *is* a statistical concept, to describe "groups." Thus it may be determined that the "national character" of the German is quite different from that of the American. Again, we are using the group as our unit of description, but purely in the statistical sense. It should be clear, without prejudicing our final decision as to the meaningfulness of the concept of "group" as superordinate to the individual, that this restricted, statistical usage of the term group is a legitimate and frequently a very useful one in social psychology.

The second usage of the term "group" takes cognizance of the fact that certain groups consist of aggregates of people who are *dynamically* related to each other, i.e., where the behavior of any one person is dependent upon his perceptions of the behaviors of all the others. In such cases it would be extremely difficult to analyze each individual in terms of his beliefs, attitudes, perceptions, habits, motives, etc., and then indicate how the resulting behavior of this individual affects and is affected in turn by the behavior of others. However difficult this may be, it would be a necessary procedure if we were interested in spelling out the various psychological events occurring in a group. But if all we were interested in were

the *end results* of these processes, then we could dispense with the individual analysis, and describe the end results in terms of "group" behavior. In other words, the *total effects* of the various individuals behaving together are measured without regard for the behavior of any single individual. Thus, for example, we can state that under certain conditions "Groups will tend to establish patterns of dominance among its members" and we can study the conditions under which this occurs for various kinds of aggregates of people and under various conditions. This is a statement involving the group as the unit of description, of study, of analysis. But the assumption still remains that *the term group is only a convenient unit in which to describe the end result of dynamic interaction among many individuals.* The term "group" as used here is also a convenient and legitimate unit of description and analysis for social psychology or general psychology.

It is the third usage of the term "group" which causes the greatest confusion and difficulty. Here it is assumed that the unit "group" is an *indivisible dynamic whole*, and that this term refers to characteristics which can never be teased out by the most exhaustive of analyses of the behaviors of the individuals comprising the group. It is further maintained that the use of such a unit will lead to laws and principles which are of a different order from those derivable from "general" psychology--the so-called laws of "group dynamics."

The use of the term "group," in modern theory, seems to rest upon the "field theoretical" approach. It is argued that just as we have long ago established that the perception of a *pattern of stimuli* contains attributes which are not derivable from an examination of each stimulus taken singly, so an aggregate of individuals contains attributes which are not derivable from an analysis of the individual members taken one at a time. This argument by analogy, it would seem, is based upon several questionable assumptions. In the first place there seems to be some confusion between the "perceptual" world and the "real" world. This danger might be clarified by examining the analogy which is frequently made between an "indivisible group" with the perception of a square made up of four dots.

We know that when the four dots are properly placed, they may yield a percept of a square, although it is obvious that the dots taken singly have no property of "squareness" about them. Could it not be said, then, that the perception of the square has *emergent characteristics* not explicable from the perceptual characteristics of the four dots viewed individually? Of course. But to argue from this analogy that in the same way a group of four people has emergent characteristics above and beyond the characteristics of four individuals existing separately is to confuse the perceptual and the physical world. In the case of the perceived square we assume that processes are set up *in the brain of the perceiver* by the configuration of the four dots in such a way that a real physical process corresponding to a square actually comes into existence. It is *not* assumed, of course, that a real square exists "out there," among the physical dots. The new emergent characteristic is real enough, it is not an "illusion,' but it exists only in the perception of the perceiver. Similarly, in the case of the four people, new "group characteristics" above and beyond any characteristics inherent in any one individual may be said to emerge when someone *perceives* these four as a group, but again, these new characteristics exist only in the perception of the perceiver. It can *not* be assumed that these emergent group characteristics exist "out there" among the four living people. If we wish to study how any one individual *perceives* groupings of people, then we can speak of the group as an "indivisible unit of perception," but if we wish to study the *behavior of the group* and not that of the observer of the group, then we cannot appeal to "gestalten" or the "field theoretical approach" to justify the assumption that an analysis of the behavior of the individuals taken singly will be inadequate.

The second questionable argument put forth in support of this usage of the term "group" as an indivisible unit attempts to answer the preceding argument. It runs somewhat as follows: Just as we can speak of field forces in the brain which account for the superordinate characteristics of visual *gestalten*, so can we speak of "social field forces." In other words, again an analogy is attempted, this time between a "brain field" and a "social field." But this, it seems

to us, is playing fast and loose with the concept of "field." The configuration of the four individuals does *not* set up processes in a "social field" which gives actual existence to the group, in any way which is similar to the brain processes coordinated with the *perception* of the four people. In the latter case there is some meaning which we can ascribe to the "field." There is a *real field* in the nervous system of the perceiver and events in one area of the field have effects throughout the field because *the intervening medium itself* is capable of being affected and of retaining such effects. But there is no such real field in which the members of the group are embedded. In the case of perceptions, in other words, we may search for the principles of explanation in the very nature of the brain medium itself and how it functions. But we cannot search for the explanation of group behavior in any "medium"—call it "social field" or whatever—for there is none! This position has been stated by Krech and Crutchfield (1948, p. 20) as follows:

> ". . . when we begin to talk about the social field, or the field of the group, as if it were a *dynamic* field, we must be particularly careful not to imply that there are any real effects going on in the medium—in this case the *spaces* between the individuals forming the group. All that can go in these spaces is the direct and simple transmission of physical energies which provide the visual, auditory and other stimuli arising from one member and impinging upon another. *All* the psychological effects take place within the individual members and *must be conditioned by the characteristics of these individuals.*"

Finally, it is necessary to point out that one of the considerations which continues to impel some theorists in social psychology to argue for the need of the supra-individual units of analysis derives from the misconception that anything less than a "group" as a unit of analysis is an "asocial" unit. But as we have pointed out at some length, the contemporary view of these other units already provides for the so-called social factors, and therefore this argument loses some of its forcefulness.

In summary, then, it appears that the following statements can be made about the social psychologist's unit of analysis: (1) in using

the common units of analysis of the general psychologist (percepts, motives, habits, individuals) the social psychologist is using *social* units; (2) in addition to these units, the social psychologist can also use the "group" as a unit of description and analysis, but in so doing he can assume that the ultimate explanation of group behavior can be sought for in the nature of the individuals comprising the group; and (3) while it is questionable, on various grounds, that any supra-individual unit of analysis is theoretically justified, the search for such a unit must avoid unexamined and unjustified arguments by analogy.

Recent Theoretical Developments

On the face of it an examination of recent developments in social psychological theory would seem to indicate that there are three major streams of development There is the work represented by the "group dynamic" approach, there is the work of the psychoanalytically trained psychologists, and then there is the approach which can best be characterized as the "perceptual approach." It should be obvious to the reader that the present writer would be somewhat biased in his assessment of the first two of these approaches From the various considerations already put forth it would appear that the third of these approaches promises the most toward the development of a mature theoretical foundation which will permit us an understanding of social behavior. The first approach, it seems to us, has yet to demonstrate that it can make any basic theoretical contributions. This is due, in part, to its insistence on the superordinate nature of the group as a unit of analysis

Group Dynamics. There are many able psychologists, notably those social psychologists who are carrying on the pioneering work of Kurt Lewin, who insist on the necessity of using the group as an indivisible unit of analysis and who are constantly seeking to establish "genuine group laws." Thus far, in our judgment, they have not succeeded in doing so, although their observational and experimental results have tremendously enriched the data upon which a complete social psychology can be built Our guess is that

they never will achieve "genuine group laws" which necessitate using the group as an indivisible unit. In considering the attempts at theorizing of these social psychologists, there seems to be one characteristic of their thinking which probably mitigates their efforts in this direction. This is the assumption, never explicitly stated, that one can write laws in terms of *one kind* of unit which simply paraphrase laws written in *other kinds* of units. In many cases it looks very much as if they would have us believe that the discovery of genuine group laws is a matter of simple translation. Kurt Lewin and his co-workers (and this is especially true of their earlier work) have made brilliant and lasting contributions to the formulation of psychological laws and techniques of analysis where the individual's psychological field was the subject of study. But this does not justify the attempt to transform these laws into "group dynamic laws" by substituting the word "group" for the word "individual" and the term "social field" for the term "psychological field" in the original formulations. It may turn out that if ever "genuine group laws" are discovered they will be phrased in entirely new terms and will necessitate concepts, properties, or variables which will be unique for those new units. Thus, for example, such "genuine group dynamic laws" may *not* be phrased in such variables as "needs" and "goals." We may not be able to talk of *group needs* and *group goals*, since it is quite possible that the concepts of "needs" and "goals" may make sense only where the *individual* is the unit of description. The failure to understand and heed this caution has resulted, it seems to us, in much rephrasing on the part of the "group dynamic" psychologists, but in very little basic theoretical contributions. In any event it is the judgment of the present writer that the recent work in perception, rather than in "group dynamics," is opening up new horizons in theoretical development.

The Psychoanalytic Approach.—No field in modern psychology has remained uninfluenced by the work and theory of psychoanalysis, least of all the field of social psychology. For the most part the contributions of modern psychoanalytic theory to our understanding of social psychology have been impressive. These contributions have addressed themselves primarily to two of the major

problems of social psychology—beliefs and attitudes, and "national character."

Psychoanalytic theory has illuminated the problem of beliefs and attitudes by stressing the *motivational* factors involved in beliefs and attitudes. In accordance with the basic philosophy of psychoanalytic theory, such psychologists as Frenkel-Brunswik have insisted that the key to racial prejudices (as one instance of an attitude) is to be found in the inner factors the individual's need and personality structures. This approach has done much to give a *functional* orientation to the problem of beliefs and attitudes—an orientation which seems, in the light of recent work (as we shall see in the next section), to be an extremely fruitful one. However, some of the psychoanalytically inclined psychologists and analysts who have addressed themselves to the problem of the motivational aspect of prejudices and attitudes have erred on the side of over-enthusiasm for the role of these "inner factors." These workers seem to suggest that certain beliefs and attitudes are *entirely* determined by personality factors and sometime rather imaginative ones. Thus they tend to speak of the "*the* personality" of the anti-Semite, or "*the* personality structure" of the quisling, or fascist, etc. Simmel (1946, p. 35), for example, declares:

> "I consider anti-Semitism a psychopathological personality disturbance, manifesting a regression to the ontogenetic as well as phylogenetic stage of the development of the ego when hatred, the predecessor of the capacity to love, governed its environmental relationships."

There is neither theoretical justification nor empirical evidence for such a position quite regardless of our assessment of the specific theory of Simmel's. A personality structure is not a disembodied "psyche" floating in a nonmaterial milieu. The anti-Semitic or the fascist or the democratic belief, attitude, prejudice, or bias is as much a resultant of the environmental stimuli as it is of the inner factors of the personality who is responding to the environmental stimuli. The over-enthusiastic psychoanalyst is here in danger of committing the same kind of error which the "pure" perceptionist

has committed—assuming the perception (or belief) is a function only of the nonmotivational (or only of the motivational) factors. A mature theoretical treatment of beliefs and attitudes must allow for both "inner" and "outer" factors—and must do so in a detailed manner.

In the area of "national character" psychoanalytic theory has also contributed much. Especially noteworthy is the theorizing of Eric Fromm (1941). His analysis of Hitler and the Nazi authoritarian character structure has much to recommend it and has served as model for many later attempts to bring to bear psychoanalytic insights to the problem of "national differences." However, here again the psychoanalytic approach has not been an unmitigated blessing. For here, just as in so many other areas of social psychology, the failure to deal adequately with the vexing problem of the unit of analysis has led to much esoteric theorizing—the net result of which has merely been to discredit (unwarrantedly) psychoanalysis among some social psychologists. A good illustration of the type of theorizing which we are criticizing here is found in Brickner's book, "Is Germany Incurable?" (1943). In this book Brickner not only treats Germany as a super-ordinate entity, but attempts to apply certain psychoanalytic concepts which were originally defined in terms of the *individual* to the entity "Germany." Thus, for example, he talks about *Germany*'s paranoia! This is, in our opinion, the "group mind" fallacy in its most virulent and pathological form!

However, whatever we may think of the over-enthusiastic claims of such analysts as Simmel and the theorizing of such men as Brickner and Gorer, it is obvious that much of the current vitality of social psychological theory is due to the influence of psychoanalysis and of those social psychologists trained in, and familiar with, the concepts of psychoanalysis.

The Perceptual Approach.—To many contemporary social psychologists the basic problem in social psychology, both from the applied point of view and the theoretical one, is the problem of attitudes. Most of the experimental work and the theoretical thinking addresses itself to the study of the development of attitudes, their

characteristics, their control, etc. Of the many approaches to this problem the one which can best be characterized as the "perceptual" approach seems to offer the most promise for the development of an adequate theory of attitudes as well as social behavior in general.

In many ways the recent concern of social psychological theorists with perception recalls to mind the story of Moliere's hero in *Le Bourgeois Gentilhomme*. It will be remembered that rather late in life, he began the study of the finer graces, including grammar. During one of his early lessons he made the astounding discovery that he had been talking "prose" all of his life without knowing it! And so the social psychologist seems to have been overwhelmed with the discovery that people have been "perceiving" all their lives without his taking cognizance of the fact. With but very little thinking it would seem obvious that all behavior, all thinking, and all action are intimately connected with perception. Before we can love or hate, be pleased or angered, make a judgment or deliver an opinion, we must see, hear, or experience—in a word, *perceive*—the object of our love, hate, pleasure, anger, judgment, etc. The individual who does not perceive objects cannot concern either the general or social psychologist. This would seem to indicate that in attempting to get an understanding of behavior we must necessarily start with a description of the *perceiving process*. But for a long time this obvious fact was neglected. Perception, as it were, was taken for granted. Today, however, the social psychologists have "discovered" perception and are insisting that we spell out, in great detail, the perceptual processes which, temporally at least, are prior to any kind of social action.

The reason for this great concern about perception does not lie, of course, in any sudden insight that perception is involved in behavior, but rather in the growing appreciation among American psychologists of the exciting and novel theoretical concepts which have been gradually developed by the perception psychologists. It is the appeal of the perceptionist's theory perhaps more than the logical importance of perception that has attracted the social psychological theorist to the study of perception. This theory, especially in the hands of the *Gestaltists*, was being applied to cover a multitude

of phenotypically different processes--perception, learning, forgetting, etc.,—and the "generalizability" of these organizational principles had a compelling attracting power for the theorist.

As was indicated earlier in this chapter, the social psychologist did not merely take over the perceptual laws and principles which had come from the perceptionist's laboratory. He re-examined them from his own vantage point, subjected some of them to further experimental investigation, and ended up by "motivating" and "socializing" the original principles. It is these new perceptual principles that the social psychologist is applying today to his basic problem of attitudes.

An Outline of the Perceptual Approach. –In this chapter we cannot hope to give an adequate systematic account of the "perceptual" theory in social psychology. The theoretical work is still in its very early stages, many of the workers in the field have not yet reached the point where they would be willing to rigidify their thinking into a systematic statement, and many of the basic theoretical constructs have not yet been agreed upon. The most that we can hope to do within the limited space of a single chapter is to present a general statement of what appears to the present writer to be a possible set of basic theoretical constructs upon which to "hook" the recent experimental work in the field. In other words, what we will present here for the most part will be more in the nature of a *program* for a theory rather than a theory itself. But again we must warn the reader that this programmatic statement represents the present writer's formulation and not necessarily the formulation to which all the "perception" theorists in social psychology will subscribe

The perceptual approach seems to suggest the following treatment of "attitudes." An attitude is to be considered as a genuine phenomenon capable of investigation by diverse methods. It is *not*, as some a-theoretical social psychologists would have it, "that which an attitude test measures." Furthermore, an attitude is not to be identified with any behavior-act, be it verbal or otherwise, which results from the attitude. In essence this means that an attitude is to be treated as a genuine, existing, *intervening variable* in the same

sense that an "atom" or a "gene" is postulated as a genuinely existing intervening variable in physics and genetics. A brief consideration of what is involved in such a view of "attitudes" will indicate that it is not as revolutionary as might be suspected at first glance. Most of the older and more orthodox definitions of attitudes already indicate that an attitude is not to be confused with either the stimulus or the response, since the most commonly accepted definitions of attitudes speak of "tendencies" or "dispositions" when referring to attitudes. These tendencies or dispositions, presumably, must inhere in the organism being stimulated and behaving, and not in the actual stimulating situation or the actual resulting gross behavior. All that the above formulation does is to define the status of an attitude in theoretically more specific and, perhaps, more fruitful, terms.

Attitudes, thus defined, can be seen immediately as specific instances of a category of intervening variables with which the perceptual and cognitive psychologists have been long familiar, i.e., "cognitive structures" or "dynamic trace systems." We can now say of attitudes what has been assumed to hold for cognitive structures in general: they are genuine phenomena that intervene between stimulus and response, they have intrinsic properties dependent upon their particular pattern of organization, they are enduring, they are in communication (in varying degrees) with other cognitive structures in the psychological field of the individual, they determine behavior--including perceptual behavior--and they obey the same dynamic organizational laws which have been assumed for all cognitive structures.

In more specific terms the position indicated above may be tentatively outlined by the following 8 statements:

(1) Most of man's perceptions are functionally meaningful. That is, they have "motivational" and "meaningful" attributes. There is no such thing, to use Gardner Murphy's term, as a "cold" perception.

(2) Many of these perceptions leave traces within the individual which persist after the stimulating condition which initiated the perception has been removed.

(3) Attitudes are built up from various perceptions and can be understood as being an organized, dynamic system of the traces of such perceptions.

(4) These traces are not static, but undergo change in accordance with the dynamic principles of organization and reorganization.

(5) As these traces undergo change, they do not remain in isolation from other trace systems, but combine and organize together with other systems into an organizational whole with its various substructures.

(6) These organized structures (attitudes), in turn, are dynamically related to other organized structures (variously called "attitudes," "needs," "personality traits," etc), are in communication with them, and affect and are affected by them

(7) Attitudes assimilate and change any new perceptions which are related to these attitudes, and as they assimilate them, or are influenced by them, the original attitudes themselves undergo reorganization.

(8) The behavior of the individual his verbal reactions, his judgments, his actions--are therefore determined by the interaction between the specific attitudes relating to the object of his action, the other dynamic systems in communication with these specific attitudes, and the immediate perceptions contemporary with the situation in which he is behaving.

The criterion of whether or not the above sketchy outline is a useful one must be its adequacy for incorporating the experimental data of psychology. In order to indicate its possible value for a theoretical ordering of empirical data we will now turn to an examination of some ot the current work being done in social psychological laboratories. We will take for special attention (and as illustrative only) the areas of a.titudes, perception, rumor, personality, and suggestion. However, it is our belief that most of the data of social psychology can be fitted into the above scheme—or some similar and better scheme.

Attitudes and the Nature of Perception.--If attitudes are to be seen as being the more or less enduring organization of various

perceptions, it then becomes of immediate importance for the social psychologist to know everything he possibly can know about perception and to use this knowledge in systematizing the phenotypical data he already has accumulated on the question of attitudes. As has already been indicated again and again in this chapter, this is exactly what a number of experimental social psychologists have been trying to do for the last several years. Among the outstanding social psychologists who have contributed to this field are the following: Asch, Bartlett, Bruner, Cantril, Luchins, Murphy, Postman, and Sherif. Almost no perceptual generalization or law or principle has been left in peace by the social psychologist, almost all of them have been investigated with a view to exploiting them in the further understanding of beliefs, attitudes, and judgments The work of Wever and Zener (1928) on the perception and judgments of weights, the autokinetic phenomenon in visual perception (Sherif, 1935), the adaptation-level theory of Helson (1947), figure-ground relationships (Schafer and Murphy, 1943), the perception of areal size (Bruner and Goodman, 1947), the principles of closure, assimilation, and contrast (Asch, 1946), the phenomenon of constancy in perception (Krech and Crutchfield, 1948), and many more, have all been examined, and used in attempting to deal with the basic problems of the social psychologist.

It may perhaps be of some value to list just a few of the suggested reformulations of perceptual laws and principles which have been proposed by some of these social psychologists, in order to indicate concretely the nature of these reformulations. The following "principles" are not listed because of any belief that they represent final or "true" formulations. Some of them, no doubt, will be revised again and again in the light of new (and perhaps more rigorously controlled) experimentation, for as is true of all new fields, the experimental techniques in many specific instances leave much to be desired. But these suggested principles do, in our opinion, illustrate the trend of current work and thinking in the field.

Thus Levine, Chein and Murphy (1942), after studying the relation of the intensity of a need to the amount of perceptual dis-

tortion, suggest that the perceptual processes occurring under deprivation may be of two kinds:

"*(a) An autistic process operating in the direction of gratification, (b) a process of mobilization of perceptual energies in the direction of accurately perceiving the means-objects in the sense of greater alertness*" (p. 291).

Bruner and Goodman (1947) after differentiating between "behavioral determinants" and "autochthonous determinants" of perception suggest the following hypotheses or "principles" based on their own experimental work:

1. "*The greater the social value of an object the more will it be susceptible to organization by behavioral determinants.*
2. *The greater the individual need for a socially valued object, the more marked will be the operation of behavioral determinants.*
3. *Perceptual equivocality will facilitate the operation of behavioral determinants only in so far as equivocality reduces the operation of autochthonous determinants without reducing the effectiveness of behavioral determinants*" (pp. 36-37).

On a somewhat different level Krech and Crutchfield (1948, p. 96) attempt to show that a reformulation in specifically social terms, of the well-established whole-part principle in perception may prove useful in understanding why we are frequently unjust or biased in our perception and judgments of people:

"When an individual is apprehended as a member of a group each of those characteristics of the individual which correspond to the characteristics of the group is affected by his group membership, the effect being in the direction of either assimilation or contrast."

And as a final illustration, Allport and Postman (1947, p. 44) in their study of rumor suggest that a "generalized law of social psychology" may be stated as follows:

"Subjective emotional distortion in the perception and interpretation of the environment can occur only in proportion to the combined effects of importance and ambiguity."

A careful reading of such principles as the above leads to several important considerations: in the first place, it would seem that to characterize much of the above work as "Investigations of how attitudes are affected by perceptions" or as "Studies of the motivational and social influences upon perception" might be somewhat misleading. Much of the current work can be seen as concerning itself with the *intrinsic* nature of perception, rather than with studying the effects of one independent set of events (motivation, need, social values, etc.) upon another set of events ("pure perception"). Here, perhaps for the first time, *the dichotomy between "motivational" and "perceptual" factors is being abolished.* It is because of this that the term "attitude" or "perception" can be redefined as an "organized dynamic system" with all that that implies. This is one of the theoretical contributions of much of this work.

In the second place the above "principles" illustrate quite convincingly one of our earlier points that "perceptions" as units of analysis cannot any longer be conceived as "asocial." The student should note how frequently such variables and terms as "means-objects," "social value," "group membership," "importance," etc., occur in *perceptual* principles. The social psychologist who studies attitudes from this point of view must know what the socially valued objects, mores, needs, group membership, and group characteristics are of his subject and his subject's society as intimately as any psychologist or sociologist who would use the "group" as his unit of analysis.

Attitudes and Dynamic Systems of Organization. To many of the social psychologists working in this field the statement that attitudes can be understood as organized dynamic systems has concrete experimental and interpretive usefulness. Such a statement suggests, among other things, that *the whole theory of the dynamics of trace systems becomes relevant to the theory of attitudes.* The various organizational laws and principles which have come to us from *Gestalt* experimentation and theory become applicable to our understanding of some of the *changes* which take place in beliefs and attitudes. The various experiments on learning and forgetting, the postulated processes of "sharpening," "leveling," and "closure," and

the concept of "Prägnanz" become some of the basic experiments and conceptual constructs for the writing of a social psychology. A recent and extremely valuable illustration of this can be found in the Allport and Postman study already referred to—the psychology of rumor. It is of interest to note that in their analysis of rumor they find it helpful to refer to studies bearing such titles as:

"Change and Decay in the Visual Memory Image" (Allport, 1930).

"Remembering" (Bartlett, 1932).

"An Experimental Study of the Effect of Language on the Reproduction of Visually Perceived Forms" (Carmichael, Hogan and Walter, 1932).

"Reproductions of Visually Perceived Forms" (Gibson, 1929).

"Über die Veränderung von Vorstellungen" (Wulf, 1922).

These studies are not merely dead lumber to give weight to a bibliography, but serve as the basic sources for Allport and Postman's analysis of such practical social psychological problems as "Pearl Harbor" rumors, "Rumor defense," "Rumor in the armed services," etc. The usefulness of this basic approach to current social psychological problems can be judged best by a careful reading of their study.

Attitudes, Perception, and Personality.—The realization that attitudes are organized structures which are dynamically related to other organized structures (such as "personality traits"), are in communication with them, and affect and are affected by them, has led to another series of research explorations. One aspect of such research concerns itself with the problem of how various personality traits are represented in the perceptual processes, while the second aspect attacks the problem in terms of larger units and seeks to investigate the intercommunication between various "attitudes" and various "personality organizations" or "structures."

The first of these are illustrated by such studies as Thurstone's factorial study (1944) Douglas' (1947) tachistoscopic study of the order of emergence in the process of perception, the work of

Asch and Witkin (1948) on space orientation, and Postman, Bruner and McGinnies' (1948) analysis of personal values as selective factors in perception. It will be remembered that Thurstone's monograph attempted an elaborate factorial study of perception to investigate the hypothesis that perceptual behavior is correlated with, or is an index of more general personality traits, and his findings seemed to indicate the value of such an approach. Douglas, who was concerned with the problem of the order and regularity of the perceptual processes, gives some data which indicate the important role which general personality development and characteristics can play in the qualitative aspects of the perceptual sequences. Asch and Witkin who were primarily interested in the process of orientation toward the upright in space (a seemingly "pure" perceptual problem) found large and consistent individual differences in the extent of their subjects' dependence upon the visual field as a basis for orientation, and they therefore suggest that:

> "While the fact of a continuous distribution of scores speaks against the existence of sharply separated perceptual types, the individuals at the extremes do differ in a fundamental way. It seems highly significant that under the same field conditions a given line is perceived by one *S* as upright, by another as slightly tilted, and by still another as very tilted. The absence of an invariable relation between the structure of the field and the mode of perception clearly indicates that a full understanding of the perceptual experience requires consideration both of the outer field conditions and of conditions within the individual" (p. 474).

These "conditions within the individual" have led to further research which (as yet unpublished) seems to indicate some correlation between *personality patterns* and the *modes of perceiving an upright in space!* And finally, Postman, Bruner and McGinnies have concerned themselves with ". . . dimensions of variation in personality which are both measurable and intrinsically important, and which can be related to individual differences in perception." The dimension of personality which they have thus far attempted to study is referred to by them as "personal interest or value," and

they have investigated the relation of this dimension to perceptual selectivity. They summarize the results of their study in the following way:

> "Value orientation makes for *perceptual sensitivity* to valued stimuli, leads to *perceptual defense* against inimical stimuli, and gives rise to a process of *value resonance* which keeps the person responding in terms of objects valuable to him even when such objects are absent from his immediate environment" (1948, p. 154).

There can be no question that the above and other work in perception justifies the assumption that the perceptual process is intimately related to, or involves, such other dynamic systems as have been labeled "personality structure" or "personality systems." Again the theoretical implications of this work have wide significance for general psychology. The time may soon come when the clinician and personality psychologist, no less than the social psychologist, will find in the study of perception the theoretical foundations for systematizing his data.

So much for our brief review of the studies which seek to relate personality data to "pure" perceptual data But, as was indicated, such a relationship logically suggests a relationship between "attitudes" (which we have suggested can be seen as organized, dynamic systems of the residuals of perceptions) and "personality structures." This approach to the study of attitudes is, of course, not unique to the perceptually-oriented social psychological theories. A very common procedure among psychologists for many years has been the attempt to find "correlations" between attitudes and such personality characteristics as "dominance," "aggressiveness," "neuroticism," etc. But the more recent work—primarily such work as that of Murray and Morgan (1945), Hartley (1946), French (1947), Frenkel-Brunswik and Sanford (1945), and Smith, Bruner and White (1947)—have gone beyond the mere statistical exercise of attempting to find correlations and have sought an understanding of the ways in which these dynamic systems interact, of how the organization of one influences and is influenced by the organization of the

other. Although much of this work is in its early stages, there can be little question but that the social psychologist's understanding of beliefs and attitudes—the beliefs and attitudes with reference to sex, peace and war, ethnocentrism, Russia, etc., to name some of the major ones thus far investigated—have been tremendously enriched. One of the immediate consequences of this approach to the study of social psychology has been the realization that the insights and techniques of the clinical psychologist are essential to the carrying out of any fundamental work in social psychology.

Attitudes, Perception, and Suggestion.—The doctrine of suggestion, prestige, or imitation as Asch (1948) has pointed out in a recent article has, for a long time, been a central and far-reaching one in social psychology, sociology, and many other social sciences. That "prestige suggestion" refers to significant and observable data cannot be denied, but that there has existed any valid theoretical treatment of these data has been questioned for a long time. Very few modern social psychologists have been content with the line of thought which seems to rest upon the assumption that "suggestion" refers to a unique type of psychological process which somehow interferes with the more rational mental processes of man and which therefore results in the adoption of "uncritical" or "irrational" attitudes. Thus in 1937 Murphy, Murphy and Newcomb protested:

> "Although the quantity of experimental work on suggestion has become voluminous, it can scarcely be maintained that our understanding has been increased proportionally. To review all the experiments on suggestion and suggestibility would be an idle and a thankless task. . . All these points have been made before; but it is remarkable that new experimental studies of suggestion continue to appear in which, instead of the more precise analysis of the behavior involved or the conditions underlying it, we are presented with a medley of results which are reported simply as scores on a suggestibility test" (p. 168).

The reason no advance has been made in the field of suggestion, in our opinion, is that no adequate alter tive theoretical formulation had been available. Lacking that, the workers in the field could do

very little more than amass more "results." However, here again, as in so many other fields of social psychology, the newer perceptual orientation seems finally to have made possible an entirely new and more satisfying theoretical treatment, at least for some of the phenomena in this field.

We might introduce the discussion of this newer theoretical approach by referring to the seventh point of our outline given on page 684. It will be remembered that we indicated there that "Attitudes assimilate and change any new perceptions which are dynamically related to these attitudes, and as they assimilate them, or are influenced by them, the original attitude itself undergoes reorganization." Now, it is obvious that any "suggestion" in order to be effective must first be perceived, and further, must be perceived as germane in some way to the already existing attitude upon which it is to have an effect. If so, we would expect, in conformity with our formulation, that any such perception (call it "suggestion" or not) will be assimilated with the already existing attitude and will, in some degree, change the attitude. Further, as is also provided for in the last point of our theoretical outline, any behavior (including a "judgment") is a function of the attitude related to the object judged and to the immediate perception of the object. Therefore consequent upon the perception of a new "suggestion" (i.e., statement about the object), the perception of the *object of the attitude* will undergo change and so will the judgment about the object. All of this is not another way of saying, in very complicated "gobbledegook," that suggestions which are effective, are effective. The above formulation says, in essence, that "prestige suggestion" or any other kind of suggestion may work *in the very same way as will any perception.* We do not have to postulate any "special force" or "unique process" called suggestion. The perception of a "prestige suggestion" adds to our knowledge about the object of our attitude, the object is consequently perceived differently, and as a result of the consequently changed organization of the attitude and *the changed nature of the object itself,* there must be a change in valuation or judgment of the object. This means two things: (1) Changes in judgment following effective prestige suggestion are just as reason-

able and obey the same laws as any cognitive change; and (2) therefore the same laws of perceptual organization and reorganization which apply to any "learned," "reasoned," or other change, apply to changes induced by "prestige suggestion."

This interpretation of "suggestion" was most clearly seen and first stated by Duncker (1938) who pointed out that:

> "If an object is found to undergo a change in valuation on being presented as the work or favorite of some admired personality, this 'suggestion by prestige' is often treated as though on an otherwise unchanged object there has been 'grafted' another value. On closer inspection, however, one may find that the object had by no means remained unchanged. To anyone but an ignorant 'snob' a 'big name' may give a new meaning or inner profile--the more readily the more ambiguous the object was in the first place" (p. 507).

This theoretical formulation of Duncker's has received a great deal of ingenious experimental verification, especially in the hands of Asch and his co-workers (see, for example, Asch, Block, and Hertzman, 1938, 1940; Lewis, 1941; Asch, 1948). In addition the work of Luchins (1945) and Asch's re-examination of the previously published work of Lorge (1936) and Sherif (1936) have contributed to the strengthening of Duncker's original formulation, especially as applied to the investigation of group influences in perception, beliefs, and attitudes. This last paper of Asch's is earnestly recommended to all students as a very compelling example of the usefulness of theory in the interpretation of data and one which spells out the fruitfulness of this perceptual and "cognitive" approach when applied to some of the old and previously unrewarding work on "suggestion."

Concluding Remarks.--It seems appropriate that a chapter on the theory of social psychology should end with a few remarks about the psychology of the theorist who has written on the theory of social psychology. In so far as such remarks are realistic they may serve as a *caveat* to the student and enable him to assess the theory more objectively.

We have tried in the last several pages to indicate the direction in which current theoretical thinking in social psychology is moving.

Obviously for one who, like the present writer, believes so strongly in the selective nature of perception, cognition and memory, there can be no pretense that the above account is not biased. We have so warned the student and the warning merits repeating. The above account was written in the belief that the perceptual-theoretical approach has more to recommend it than any other approach. This belief is based on some observations, some familiarity with the literature in the field, and in large part is due to a specific faith or philosophy of science. Fundamentally the present writer is enamoured of (or fixated upon) the perceptual approach not only because it seems to him to have proved itself so fruitful in social psychological theory and experimentation, but also because it seems to offer so much hope for the unification of the various fields of psychology.

The particular perceptual approach which we have attempted to describe in this chapter has implications for the work of both the social and "general" psychologist as well as for the clinical and other applied psychologists. This approach, by using the general psychologist's principles and by demanding that the social psychologist familiarize himself with the general psychologist's laboratory, his principles, his experimental techniques, and his mode of thinking may prove to prevent fission between the general psychologist and the social psychologist. It may give the social psychologist some desirable training in the techniques of good experimentation, it may give him a sense of continuity and generality of his science, it may give him some appreciation of the value of "pure" research and, above all, it may give him a set of principles from which he can build a scientifically adult theory for his own special concerns. And, as we have suggested, the very same process may occur in the case of the clinical psychologist, and for the very same reasons.

But this approach also, it must be remembered, forces the general psychologist to revamp his thinking and his behavior as a scientist. It forces him, in other words, to observe the society around him. It makes the observation and study of the social world we live in—the customs, mores, social values, institutions—just as relevant and just as essential as the study of the physical and biological world. Many general psychologists have long assumed that

the physicist, the neurologist, the geneticist, and the physiologist had important data which the psychologists could use in describing the stimulating environment and behavior of their experimental subjects. They must now realize that the sociologist, the anthropologist, the economist, and the political scientist also have such data. *Any* psychologist, no matter what his own research concerns may be, must not only have a good description of his physical stimuli—colors, brightnesses, maze-patterns, or whatnot—but also a good description of his "social stimuli."

Both of these consequences (that the social psychologist must concern himself with the "laboratory principles" and that the general psychologist, with the "social phenomena") suggest the real possibility that we may eventually develop a useful and *generalized* set of psychological laws which will serve all psychologists alike.

It must be admitted that this desire for generality in scientific laws, the search for a universally valid systematization may be, as Gardner Murphy phrases it, an indication of the paranoic approach to science. The world may very well be so constructed as to make it impossible to find any single valid "system." Different data and different events may have their own laws, and the laws of perception and learning may differ from the laws of attitudes and suggestion But such a hebephrenic view of the world is repugnant to the present writer's systematized beliefs, attitudes, and faith. It is for all these reasons that the present writer and many other social psychologists have put their faith in the perceptual approach. We see it not only as the most fruitful one for social psychology, but for all of psychology; and we see it as consistent with the belief that it is meet and proper for scientists to seek the generalized laws which cut through and explain phenotypically different data in terms of a unitary conceptual framework.

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CHAPTER 15

ABNORMAL PSYCHOLOGY

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Those who have recently completed courses in abnormal psychology might deem it more useful to devote a chapter to *fact* rather than theory in abnormal psychology. Furthermore, if the term *fact* be taken literally, such a chapter would be much shorter than one devoted to adequate coverage of the merely theoretical aspects of the field since almost every topic, every syndrome, and every working concept bristles with theoretic issues. There are theories of synesthesia, schizophrenia, aboulia, audiogenic seizures, Mongolian idiocy, and the illusion of *deja vu*. To deal with theory in abnormal psychology at this level of specificity would be to write another book on the subject. Being restricted to a single chapter necessitates seeking a different level, one that obviates consideration of circumscribed theories and the minutiae of the subject matter of abnormal psychology.

In recent years the field of abnormal psychology has been much influenced by developments in the very active field of clinical psychology. As will be indicated later, in some respects the latter field may be regarded as dealing with the practical applications of the former. At all events the two are so closely related that discussion of theory in one impinges on the other. Within restricted limits, then, the present chapter will be concerned with theory as it relates to both fields even though this complicates the task of exposition. However, ruling out all consideration of clinical issues would make for a distorted and misleading account of the present status of the field of abnormal.

Quite obviously, as was suggested at the beginning, it will be impossible to do justice to *all* theoretic issues in a single chapter devoted to what amounts to two fields of psychology. Instead an

attempt will be made to organize some of the more pervasive theoretic considerations around a few central concepts. These concepts will be used as focal points for more systematic exposition of some salient theories. Some important theories will have to be slurred over or altogether neglected as a consequence of this mode of treatment; but it is to be hoped that there will be a compensatory gain in unity of organization as well as in the exposition of the ways in which abnormal and clinical psychology are related to many of the other fields discussed in this volume.

Three concepts will serve as points of departure for exploration of theoretic content belonging to the field of abnormal psychology and the contiguous field of clinical. First the concept of abnormality itself will be examined. Then the concept of mental conflict will be used as a second point of departure. In the last section the concept of psychotherapy will be made to function as a third point of departure in order to round out this survey of theoretical perspectives.

The Concept of Abnormality

An historico-critical survey of abnormal psychology involves the introduction of numerous and shifting as well as occasionally conflicting perspectives. This is another way of saying that the concept of abnormality does not lend itself to facile formulation.

As a first approximation one is tempted to dispose of the relationship between normal and abnormal psychology by reference to an analogy between physiology and pathology or normal anatomy and teratology. The latter reference suggests one meaning of abnormality as including the freakish, the atypical, the dysgenic, and the monstrous. A perspective of this kind harks back to antiquity. The ancient Egyptians, as Born (1947) has pointed out, "knew the difference between pygmies (normally small races) and true dwarfs who were pathological cases." This difference foreshadows distinctions the implications of which continue to be the subject of contemporary debate.

Is Hypnotizability Abnormal?—Quite obviously the content of abnormal psychology will depend upon who and what one regards as abnormal. Historically, this issue came to a sharp focus in the

period when the foundations of abnormal psychology were being established in the last two decades of the nineteenth century. The specific problem had to do with the "normality" of hypnotizability. Charcot, it will be recalled, had laid down the dictum that susceptibility to hypnosis was pathognomonic of hysteria

This dictum was called into question by Liébeault and Bernheim who had found no necessary relationship between ease of hypnotizability and evidence of an hysterical diathesis. In their opinion hypnotizable persons were not abnormal in the sense of being restricted to those classified as psychopathic or neuropathic. In fact, they had little trouble in inducing hypnosis in many rugged, emotionally stable French peasants. Whether hypnosis as a circumscribed psychological topic falls within the purview of normal or abnormal psychology is thus a question to be answered in one way by those following the Charcot tradition and in a different way by those of the Bernheim tradition.

Deviation vs. Distortion. Abnormality may be viewed either as a deviation from some norm or standard or else as a distortion of a norm or standard The former suggests quantitative and the latter qualitative changes. Superior retentiveness as well as inferior retentiveness or hypermnesia and hypomnesia, respectively, serve as convenient instances of the one kind of change just as distortion of memory or paramnesia exemplifies the other kind

In terms of a neat classificatory scheme, it would be tempting to allocate quantitative deviations to the field of differential psychology and the qualitative distortions to abnormal psychology The actual development of these fields, however, as a little reflection will show, has failed to respect any such lines of demarcation. Instead there has been overlapping and telescoping of interests and viewpoints. Feeblemindedness, for example, has fallen within the purview of both differential and abnormal psychology Of course, feeblemindedness due to demonstrable tissue pathology, as in cases of secondary amentia, obviously falls within the scope of abnormality irrespective of the precise connotations of the concept. In other words, there never has been any uncertainty about the unambiguously pathological being abnormal. Consequently, for some psychologists the field of

abnormal psychology has come to be coterminous with the field of psychopathology. An excellent example of this viewpoint is supplied by Maslow and Mittelman's (1941) textbook.

Place of Psychopathology.—A different orientation is supplied by those who refuse to equate psychopathology with abnormal psychology. In the volume by Conklin (1935), for example, it is expressly stated that abnormal psychology is broader than the field of psychopathology. Like many other abnormal psychologists Conklin devotes space to such topics as dreams and the intellectual brilliance of genius. Such topics are patently devoid of any necessary relationship to mental disorder. They have to do with the abnormal only in being deviations from some frame of reference regarded as normal.

An additional instance of this more catholic and flexible outlook is to be found in those who include parapsychology in their treatment of abnormal psychology. In Conklin's text there is a whole chapter devoted to what he called "spiritistic" phenomena with discussions of fraudulent mediums, crystal gazing, and telepathy. This means that, if Conklin's standpoint be adopted, what was formerly classified as psychical research can be subsumed under the caption of abnormal psychology. Seemingly the unusual, the paradoxical, the occult as well as the frankly psychopathic can thus be brought within the orbit of the total field.

The Statistical Criterion. It was just suggested that abnormal and differential psychology, viewed as distinctive fields, overlap when the statistical criterion of abnormality is applied independently of other possible criteria. In terms of this orientation the terminal areas of frequency distributions become the areas of abnormality and the intervening area is regarded as pertaining to the normal.

Depending upon the rigor of one's standards the scope of normality may be allocated either to the interquartile range or to one sigma above and below a central reference point. In practice this approximates equating the normal with the average and is illustrated by such common phrases as "normal height" or "normal intelligence." Terman (1916) classifies IQ's ranging from 90 to 110 as indices of "normal or average intelligence," whereas Wechsler

(1941, p. 40) suggests a range from 91 to 110. What this latter range includes is the middle 50 per cent or the IQ scores falling within the area from −1 PE to +1 PE. In many respects this conventional approach to the concept of normality is more congruent with problems of differential psychology than with those of psychopathology.

Paradoxical connotations have obtruded themselves because of the difficulties of employing a word like *abnormal* in an exclusively statistical setting. To say that men like Aristotle, Shakespeare, Galileo, Goethe or other creative geniuses were "abnormal personalities" tends to make for misunderstanding, because the common-sense implications of the adjective persist in throwing a nonstatistical halo around the technical use of the word. Nor is the substitution of a phrase like "deviant personalities" a satisfactory corrective. Here, too, as in the case of *abnormality*, the connotation suggests deviation from the average in one direction only with almost complete disregard of the opposite direction. One upshot of this state of affairs has been the tacit recognition that even in terms of the statistical criterion of abnormality only psychological deficit falls within the scope of this criterion.[1]

Genius as Psychopathy.—An unqualified statistical approach to the concept of abnormality also raises another troublesome issue by making mediocrity the standard of normality. The average becomes transformed into a standard of excellence when "C" grades are classified as "normal" and "A" grades as abnormal. Such a conclusion is reminiscent of Max Nordau's (1905) old teaching regarding the intimate relationship between genius and insanity.

Implicit within such a teaching one may detect a belief that in the biological nature of things extreme divergences from central trends may be maladaptive. Both hypertrophy and atrophy connote the pathological. Neither circus giants nor circus midgets are to be deemed "normal." It is as if the giant's physique has grown too ponderous to make for adjustive efficiency so that if his descendants were to become progressively more ponderous they, like the dino-

[1] For additional limitations of the statistical criterion the article by Wegrocki (1939) should be consulted.

saurs of old, would fail to survive. However, in terms of Galton's law of filial regression no such increments of bulk would be visited upon successive generations of a dynasty of giants. Instead there would be a shift back to some group average.

This raises the interesting but as yet unanswered question as to whether there may be an optimal positive deviation from such an average so that more extreme deviations would constitute handicaps rather than assets. That one can be too tall or too heavy to be healthy is obvious. Whether one may be too intelligent or too self-controlled to be maximally efficient may not be so obvious. Nordau to the contrary notwithstanding this is still a theoretic construct not yet amenable to operational verification.

Within the limits of the upper reaches of the kind of intellectual competence symbolized by IQ scores the available evidence has actually run counter to Nordau's thesis. As the studies directed by Terman (1947) have shown, children with high IQ's grow into well-adjusted, stable adults. It may be argued, however, that Terman's "gifted" children were not really "geniuses" in Nordau's sense. High scores on conventional tests of intelligence, it might be alleged, merely measure competence to deal with conventional abstractions rather than capacity to be genuinely creative by breaking with tradition and bringing something novel into the world.

It should also be pointed out that the general drift of contemporary clinical and experimental evidence is not in line with Nordau's theory. As Goldstein and Scheerer (1941) have shown, there is a positive relationship between brain pathology and loss of capacity to be an effective abstract thinker. Both volitional as well as cognitive factors are involved in creative thinking so that on *a priori* grounds it is unlikely that the impairment of volitional control associated with psychopathy would actually facilitate such thinking.[2]

The Adjustive Criterion.—As was just implied, integrity of mental functioning is a prerequisite for effective problem solving behavior. Recognition of this fact is implicit in the concept of maladjustment as the chief criterion of abnormality. In terms of this

[2] For more detailed exposition of the issues involved "The Survey of Psychological Deficit" by Hunt and Cofer (1944) should be consulted.

criterion deviations from a norm are abnormal only if they militate against adequate handling of problems incident to being a productive or useful member of society. The theoretic implications of this position are by no means simple. In a recent discussion of the topic "What Is Normal Behavior?" Mowrer (1948) projects these implications against a philosophic background. He introduces an axiological factor by pointing out that the notion of value is tucked away in the concept of normality as efficiency. To say that normal behavior is *better* than abnormal behavior is one way of making this implication explicit.

What makes one kind of behavior *better* than another? Answering this question calls for an excursion into the realm of value theory. Mowrer does this by borrowing a suggestion made many years ago by the philosopher, Ralph Barton Perry. According to Perry, there are three ways of envisaging the value of a particular action depending on whether it is designated as *adaptive* or as *adjustive* or as *integrative*. It may not be too much of a distortion of Perry's thinking to interpret these as biological, egocentric, and ethico-social criteria, respectively. Actions that enable the organism to survive would thus be classifiable as adaptive actions. Those that cater to the individual's comfort or pleasure would fall into the category of adjustment. Those making for a unified reconciliation of the clash between duty and pleasure-seeking impulse would exemplify the concept of integrative behavior.

Mowrer ventures to articulate these three levels of behavior with three historic suggestions regarding the nature of learning. For expository purposes it may be advisable to change his nomenclature slightly without changing the drift of his thinking. What he seems to be proposing is that learning theories may be grouped into a threefold classification.

(1) those that stress contiguity in experience as the crucial factor;

(2) those which focus on the consequences or effects of behavior;

(3) those which emphasize insight or rational grasp of basic principles as the crux of the learning process

As Mowrer sees it, *associationism* and its variants would exemplify the first group, *hedonistic* theories like the law of effect the second, and *rationalistic* or insight theories the third. Although he grants that the parallelism is not perfect, he nevertheless perceives an approximate correspondence between adaptive behavior and associative learning at the biological level. Analogously adjustive behavior is seen as a function of hedonistic learning at what was just referred to as the egocentric level, while integrative behavior reflects rational learning at the level of ethico-social insight.

Levels of Behavior.--Those familiar with the history of psychology may recognize the foregoing application of the concept of *levels* of behavior as one more variant of the persistent problem of personality organization. From the ancients to the moderns this approach appears in various guises One thinks of Aristotle's vegetative, appetitive, and rational souls as an early use of the concept of levels. Neurophysiological references to spinal, midbrain, and cortical functions reflect a later dependence on this concept. It is also revealed when contemporary Freudians talk about id, ego, and superego factors. Even the Gestaltist has his global, differentiated, and integrated levels of development or reaction

Irrespective of the nomenclature used these different viewpoints all suggest the need to recognize the existence of a hierarchy[3] of functions implicit within the complexities of behavior. They also imply that without such hierarchical control the efficiency or health of the organism would be jeopardized. In the language of classical psychology mind or personality has often been referred to as a *unitas multiplex*---a manifold unity. When this unity is threatened by disruption of hierarchical control, personality disintegration supervenes.

Thus in terms of this broad, historical perspective one can see a relationship between any doctrine of mental or neural levels and the emergence of such a field as abnormal psychology. The latter field, in other words, is concerned with failure to maintain the "unitas"

[3] An application of this hierarchical concept to the more restricted problem of purely cognitive functions was introduced by Professor Spearman (1927) some twenty years ago in his classification of theories of intelligence into monarchic, oligarchic, and anarchic theories

of the "multiplex" so that for those responsible for its emergence the big problems of abnormal psychology in the later decades of the nineteenth century had to do with multiple personality, the nature of dissociation, and kindred manifestations of disunity within the "manifold" of mental processes.

Whatever hampers personality integration can thus be viewed as relevant to the field of abnormal psychology. Neural lesions, infectious processes, toxic disturbances, endocrine imbalance, and faulty nutrition are all involved in the conventional list of etiological factors responsible for inadequacy and disruption of personality integration. This list is, of course, concerned with the so-called organic or structural disorders. In terms of the traditional structure-function dichotomy a list such as this reflects the physicochemical approach to behavior problems or the conceptual framework of the neuropathologist. For him personality disintegration, maladaptive behavior, amnesic episodes, and other dissociative phenomena are to be understood as consequences of the disruption of the biological matrix of normal functioning. The latter functioning, psychologically considered, presupposes the maintenance of the kind of internal environment symbolized by the concept of homeostasis

Homeostasis and Abnormality.—As Cannon has pointed out, despite rather wide variations in external environment, the homeostatic factors in the organism keep the internal one relatively constant. Body temperature, for example, is a constant for the normal individual even though he is exposed to extreme thermal fluctuations. Other homeostatic constants are represented by blood sugar, blood calcium, and similar biochemical constants.

It is largely in terms of such a homeostatic frame of reference that medical diagnosis proceeds. Physiologically considered, the normality of the organism is determined by means of such reference points. This means that the medical practitioner can detect illness by doing his professional thinking in terms of such biochemical and biophysical concepts. They suffice to enable him to recognize a wide diversity of abnormal conditions from myopia and cataract to typhoid fever, clubfoot, and cancer. There is no need for him to bring "personality" factors in the way of motivational dynamics,

patterns of culture, and levels of aspiration into his clinical appraisal. Such biosocial factors in the life of man can be shunted out in the sense that a knowledge of "normal" anatomy, physiology, and body chemistry is enough to render routine medical diagnosis possible.

This excursion into the nature of medical diagnosis is of some relevance for the abnormal psychologist because of its bearing on two important theoretic issues. The first one has already been touched on in passing, but merits more elaboration in the present context. We have reference to the problem of deciding whether the shift from mental health to mental disease involves a dichotomous classification or a transition along a continuum.

Both psychometric studies of intelligence as well as psychoanalytic teaching made it appear likely that the shift in question was a gradual transition. No convenient hiatus in the sequential distribution of intelligence test scores served to mark the boundary between normal intelligence and amentia. Nor did the Freudians find "normal" individuals to be altogether free from the operation of mental dynamisms presumably responsible for neurotic symptoms. One outcome of this kind of emphasis was the doctrine that mental abnormality was not to be viewed as qualitatively different from the normal.

The Dichotomous Approach.—Writers of textbooks on abnormal psychology, even though they differed on numerous issues, tended to be in agreement on one: abnormal behavior represents a difference in degree and not in kind. Often they added that "popular" as opposed to "scientific" psychology fails to realize this "fact" as evidenced by common-sense pigeonholing of people into mutually exclusive categories indicated by saying that a man is either sane or insane, intelligent or stupid, normal or abnormal.[4]

Gordon Allport was among the first to question this particular repudiation of common-sense psychology. Part of his questioning was an outgrowth of his realization that what had happened within the field of psychopathology was at variance with developments in

[4] Such pigeonholing has been called *rubricizing* by A. H. Maslow (1948). His discussion of this process has a direct bearing on some of the theoretic issues being considered even though his article is not primarily concerned with abnormal psychology.

the field of ordinary pathology. In other words, for the medical man dichotomous thinking is not yet legislated out of scientific existence. The normal-abnormal dichotomy continues to be intellectually respectable in connection with diagnostic problems. A physician is not regarded as outmoded or unscientific because he argues that a given patient either has typhoid fever or is free from typhoid. Such diagnostic procedures obviously presuppose the legitimacy of a qualitative distinction between such correlatives as health and disease or normality and pathology. The distinction is not just a matter of degree.

For Allport (1937, p. 76) on *a priori* grounds, it appears unlikely that disjunctive concepts should be relevant to the thinking of the internist and irrelevant to the thinking of the psychiatrist. Stated a little differently, this is tantamount to asking whether the basic conceptual framework underlying organic pathology is radically different from that in which psychopathology lies embedded. If it be granted that with respect to brain tumors, for example, a neurologist is not doing violence to fact by arguing that neoplasms are either present or absent from given patients, then why should one insist by implication that it is unscientific for the psychiatrist to view some people as mentally disordered and others as mentally healthy? If organic pathology accepts the legitimacy of mutually exclusive categories, why should psychopathology be different?

From Normal to Abnormal. - Another way of viewing this problem can be introduced by asking whether abnormal psychology as a separate field deals with phenomena essentially like those studied in the field of general psychology except that they differ quantitatively. If so, then by imaginative extrapolation, so to speak, the student of one field might master the other without having to study it directly. This would be true in the same sense as the student of "abnormally large" triangles could understand the geometry of those of "normal" size without bothering to deal with them directly. This seeming *reductio ad absurdum* is justified in a discussion of psychological theory because it is by no means uncommon for students of psychopathology to note a more or less obvious relationship between

psychotic and neurotic symptoms and the characteristics of the normal person.

This is what Allport has reference to when he asks, "Is the normal personality simply an undistinguished edition of the mentally diseased?" An affirmative answer to this question might be supported by those who see analogues to unambiguously psychopathic behavior in many commonplace normal experiences. Everyday difficulties in recalling names or telephone numbers are compared to post-traumatic amnesias. Dejection following academic failure is seen as a reduced version of a psychotic depression. A businessman's vacillating uncertainty regarding the desirability of a certain investment is compared to the ruminative tension states of the psychasthenic patient. Manic excitement is seen as having some resemblance to crowd behavior after a football victory or the behavior of people trapped in a hotel fire.

There is no question about the didactic usefulness of such analogies and comparisons. They serve to clarify the meaning of the technical terms used to describe the symptomatology of mental disorder by enabling the student to relate what he already knows about normal people to what he is trying to learn about abnormal ones. But are they more than didactic devices? Are they to be viewed as pointing to abnormal trends to be found in all normal people? If so, then they are not mere analogies, for analogies imply differences and the temptation to neglect them in the heat of debate is what renders argument by analogy a hazardous intellectual instrument. Accordingly, it is altogether in order for the student of abnormal psychology to inquire whether important differences are being overlooked as he makes his way from the normal to the abnormal *via* the analogical route.

Those who envisage the normal-abnormal dichotomy as spurious, because for them there is no sharply defined zone of cleavage but a continuum of transitional change, are likely to maintain that even if these customary comparisons be classified as analogies, the resemblances predominate over the differences. The latter are disposed of as no more than matters of degree. Presumably ordinary forgetting is like post-traumatic amnesia except that the blocking

is either not as intense or as frequent or as persistent in the former as in the latter. In similar fashion, to be consistent, ordinary fears would have to be viewed as less intense phobias and the self-respect of the average man as a reduced version of the paretic's delusions of grandeur.

It might well be asked whether *any* abnormal symptom exists which is not explicable as an intensive deviation from the personality make-up of the normal man. If so, then the relationship of normal to abnormal psychology would be different from the relationship between general psychology and child psychology. As a moment's reflection will show, an interesting divergence of emphasis has characterized the growth of these fields. In child psychology the student is warned over and over again not to be misled by obvious discrepancies in stature into regarding the child as a miniature adult. In terms of this perspective the mind of the adult is not just a stepped-up version of the child's mind. To a large extent adult psychology and child psychology are thus presented as involving a sufficient number of *qualitatively* unique features or patterns so that each field may be regarded as *sui generis*.

In being introduced to abnormal psychology, however, the student is warned over and over again not to be misled by obviously unique features of abnormal behavior into thinking that such behavior represents a difference in kind with respect to normal behavior taken as a standard. Seemingly each of these two fields is not to be regarded as *sui generis*, and the facts of one ought to be transposable to the area of the other after appropriate alteration of the magnitude of each fact. A clarifying analogy for this view might be presented if one were to compare the work of the abnormal psychologist to that of a tailor specializing in making suits for men hard to fit and that of the general psychologist to the work of a tailor who limits himself to fitting suits to men of normal or average build. In terms of this analogy one could readily learn to do the work of the other.

From Abnormal to Normal.--The chief reason for discussing this problem is bound up with the fact that it has become a not infrequent practice on the part of some experts in psychopathology

to talk and write as if what they found to characterize their abnormal patients may also be attributed to normal people. Some psychoanalysts, for example, venture to carry over their interpretations of the dynamics of neurotic behavior to the behavior of the normal person without direct study of normal personalities. It is as if the secrets of the consulting room could be universalized to apply to humanity in general. *Every* child is thus saddled with the Oedipus struggle. If this general procedure be sound methodologically, then the general psychologist would be justified in venturing to pose as an expert in psychopathology without dealing with the mentally disordered directly, since he, too, has access to these allegedly universal dynamic principles. And yet, strangely enough, this has rarely, if ever, taken place. At least it has not taken place any more than the specialist in general physiology has been prompted to usurp the duties of his colleague in general pathology without first familiarizing himself with phenomena unique to the latter subject.

The facts and theories germane to pathology are not automatically applicable to normal physiology without suitable qualification and vice versa; hence the justification for a division of labor. This is not to say that there is no interchange of methods and concepts between the two fields It is rather to say that such interchange calls for *critical* evaluation of likenesses as well as differences. Furthermore, it might be well to note that ordinarily the pathologist secures a sound background in normal physiology before embarking on his specialty. Unfortunately, many psychiatrists and psychoanalysts do not follow this precedent by first getting a solid grounding in normal psychology. Their first professional exposure to technical psychological issues is in terms of the morbid, the maladjusted, and the atypical. In the sister field of clinical psychology the hazards of this educational procedure are already recognized and those responsible for the training of clinicians are insisting that they be psychologists first and clinicians subsequently.

The Concept of Mental Conflict

In routine medical thinking, it will be recalled, the implicit or explicit utilization of a homeostatic frame of reference enables the

practitioner to discriminate the normal from the abnormal, or health from disease. A cardiologist or ophthalmologist can practice his specialty in any part of the world irrespective of ethnic, cultural, or status differences existing among his patients. Neurocirculatory difficulties or refractive errors are discovered by reference to homeostatic standards having universal as opposed to parochial applicability. Mitral stenosis or astigmatism as symptoms of cardiac or ocular abnormality are not unique to a particular culture or a given people. They occur all over the world; hence the facility with which cardiologists and ophthalmologists can render their respective professional services to patients from any country of any religious persuasion and any social level. The same may be said of any other medical specialty with the exception of psychiatry. The reasons for and the implications of this exception are of direct concern to the student engaged in examining the theoretic presuppositions of abnormal psychology

Idiographic Factors.—More than any other medical specialty, psychiatry is forced to come to grips with idiographic in addition to nomothetic concepts. The psychiatrist's patients, especially those classified as victims of functional mental disorders, have to be viewed as troubled personalities and not as mere biological organisms. Because personality to so large an extent is a product of social tradition and social interaction the psychiatrist, unlike his colleagues in the other medical specialties, cannot afford to neglect a social science orientation. What he calls mental conflict is a clash between societal demands or expectations and rival individualistic demands or longings. To ward off a possible misinterpretation of the full scope of the meaning of the word *conflict* expressed in the last sentence it might be well to digress at this point in order to get our bearings in terms of the perspective of history.

As a pivotal concept in the thinking of psychopathologists the idea of mental conflict may be traced to the rival views of Janet and Freud. Janet stressed "lowered psychic tension" as primary and conflict as secondary in the etiology of functional disorders. As he saw it, reduced energy resources weaken the patient's capacity to cope with conflict situations a normal person could dispose of with-

out signs of psychopathy. On the other hand, Freud stressed the exhausting effect of conflict with consequent disruption of control and efficiency. As Dalbiez (1941, p. 69) has phrased it, in Freud's thinking we have "deficit resulting from conflict," whereas Janet's thinking gives us "conflict resulting from deficit."

In general, the concept of mental conflict requires the modern psychiatrist to be familiar with the impact of social institutions on personality development. When he refers to a *normal* personality he is apt to think of one who, to employ a stock psychiatric phrase, is adjusted to reality. And by reality he is not referring to the world as described in terms of the facts of astronomy, geology, chemistry, biophysics, physiology, and other natural sciences. Instead he is referring to the world of ethicosocial reality having to do with such diverse aspects of daily living as job security, being loved and having somebody to love, feeling socially accepted rather than rejected, living up to community codes with respect to standards of ethics and etiquette, and—in brief—the never-ending struggle to be good and make good.

The world of ethicosocial reality is seemingly devoid of the cultural equivalents of the homeostatic constants of biophysical reality. Presumably each age and each culture stumbles on its own pattern of the good life so that each person has to work out modes of adjustment to accord with norms of conduct endorsed by his people. The consequent relativity of ethicosocial norms renders it impossible to set up any absolute criteria of personality balance.

In the light of this perspective there would be reason to contend that there are as many different kinds of normal personality as there are distinctively different kinds of patterns of culture. From this it would follow that the psychiatrist's sphere of professional usefulness would be limited by his own cultural restrictions, since not even the expert anthropologist can have adequate understanding of more than a very few cultures.

Abnormality and Cultural Relativity.—As was just suggested, terms like functional mental disorder and maladjusted personality reflect a recognition and acceptance of the doctrine of cultural relativity. In the light of this doctrine a person is to be regarded as

normal to the extent that his behavior is congruent with the demands of ethicosocial reality rooted in the folkways of his people. Such a person is said to be *adjusted* to reality. Community standards or cultural norms are used as criteria for appraising the adequacy of such adjustment. This is brought out very clearly by Hinsie and Shatsky (1940, p 372) in their discussion of what they take to be the psychiatric meaning of a phrase like "psychically normal person" In their own words:

> "From the psychiatric point of view, a psychically normal person is one who is in harmony with himself and with his environment He conforms with the cultural requirements or injunctions of his community. He may possess organic deviation or disease, but as long as this does not impair his reasoning, judgment, intellectual capacity and ability to make harmonious personal and social adaptation he may be regarded as psychically sound or normal"

If the latter definition be accepted, then the normal person is the conventional one, and abnormality may be equated with unconventional deportment As a consequence, there can be no universally applicable criteria of normality, for, like sartorial fashions, conventions fluctuate from age to age and from culture to culture A person adjusted to one culture would be maladjusted when plunged into a very different one

Functional disorder would depend not merely on the behavior trends of the patient, but also on the patterns of convention prevalent in his community It might even be that in given cases psychiatric treatment would call for little more than transferring a patient from one cultural milieu to another Such a procedure is, of course, often recommended when the patient's troubles are attributable to his inability to learn to feel at ease in what is for him an alien environment. Americans forced to live in isolated Chinese villages or a pious Italian Catholic compelled to settle among non-Catholic Norwegians are convenient instances of the sort of culture conflict justifying the latter type of procedure

There is thus considerable justification for the contemporary psychiatrist's emphasis on congeniality of cultural norms as an

important factor in warding off functional disorder. Nevertheless, careful consideration of the theoretic presuppositions and consequences of the concept of abnormality emerging from this emphasis may render it difficult to endorse this concept without qualification.

Thibaut has called attention to the fact that the statistical criterion of abnormality is not unrelated to the one which makes maladjustment a function of failure to abide by conventional standards of behavior. Customary behavior is exemplified by what the "average" person in a given culture does under a specified set of circumstances. Conventional behavior is thus apt to be in line with what has been called the "social reaction-average." However, despite the telescoping of the two concepts, they are not to be viewed as identical. As Thibaut (1943, p. 339) has put it: "The relation of the 'adjustment' concept to the statistical is transitive, i.e., the first implies the second, although the converse is not true."

Normality as a Value Concept.--In terms of a democratic political philosophy it might be unwarranted to question the doctrine of cultural relativity by suggesting that a given culture might be superior to another. Each people is supposed to be entitled to work out its own cultural destiny, and it is not for the outsider to approve or disapprove. The presumption is that the cultural pattern in question satisfies the needs of the group involved.

Cultural pluralism thus appears to be a necessary corollary of the democratic principle of self-determination. Consequently, it might be a violation of the latter principle for a psychiatrist to stigmatize a given culture as "abnormal" because of its presumed distorting effect on the personality development of its adherents. For him to do so might expose him to the charge of introducing value judgments derived from his own culture and imposing them on an alien one.

Moreover, since value judgments are supposed to be shunted out of the universe of scientific discourse,[5] to venture an opinion regarding the relative merits of different cultures would constitute

[5] A critical discussion of this and related issues is to be found in the study by Kohler (1938).

violation of a canon of science. Such opinions, it is sometimes held, belong to ethics or esthetics, but not to science.

In actual scientific work, however, it is impossible to maintain such rigid compartmentalization in one's thinking. Despite protestations to the contrary, value judgments keep obtruding themselves. It is hard to deny, for example, that health is preferable to disease or rule by law to rule by violence or job security to insecurity or straight thinking to crooked thinking or a machine that runs well to one that breaks down. With respect to the concept of mental health or mental efficiency it may thus not be unscientific for the psychopathologist to raise the kind of issue under consideration here.

Equating normal behavior with conventional behavior may make for clinical convenience; but, as has already been suggested, it may also be obscuring underlying contradictions and psychiatric paradoxes. It suggests that, unlike ordinary physical illness, mental illness is subject to the vagaries of cultural divergence. Typhoid fever is recognized as a disease all over the world by any competent diagnostician, and yet similar universality of diagnostic assurance would not hold for the psychiatrist's efforts to identify functional maladjustment.

The mentally ill of one culture might actually be the mentally healthy of another. The sane of one culture might be the insane of another, and vice versa. This is the possibility which Glover[6] seems to have had in mind when, with reference to the psychiatric criterion of normality as a matter of adjustment to the realities of group standards, he contended that "normality may be a form of madness which goes unrecognized because it happens to be a good adaptation to reality." In a militaristic culture, for example, the ardent pacifist would be maladjusted or regarded as abnormal. Similarly, to cite another instance, in a community which condones lynching the lone dissident who refuses to join the mob would be failing to act "normally" with respect to the folkways of this subculture. In less hypothetical terms one might ask: Were the Germans who refused to conform to the Nazi "social reaction-average" psychopathic and the Nazi conformists normal?

[6] Glover, E., as quoted by Thibaut (1943, p. 338).

In everyday clinical work the notion of a well-adjusted personality carries with it the tacit assumption that prevalent ethicosocial standards merit psychiatric approval. At all events this appears to be the case whenever a patient is regarded as maladjusted because he is at odds with society. The clinician is apt to classify the maladjustment as one more instance of the "they're-all-out-of-step-but-Jim" type of egocentric thinking. To safeguard its own welfare, so it is assumed, the group has to be regimented in various ways, and for the individual to balk at such regimentation signifies maladjustment on his part. His maladjustment may actually be referred to as antisocial or as asocial.*

Impact of Folkways.— It ought now to be obvious that abnormal psychology is forced to consider issues ordinarily regarded as belonging to social psychology. The latter field is directly concerned with the bearing of institutional organization on personality development. In particular, the institution of the family, as the surrogate of the Great Society, stands out as the chief influence in the infant's acculturation or socialization. As seen by some students of personality development, this impact of family folkways on the growing child may be so influential as to convert him into an adult "carrier" of the culture of his people. In terms of this perspective such students actually define personality as "the subjective side of culture."

The implications of such a perspective for abnormal psychology have been rendered especially prominent by those who envisage neurotic behavior as a product of conflict between assimilated family standards and antithetic individualistic urges. Such a view results in references to a clash between ego needs and superego demands or the tyranny of society over the spontaneous impulsiveness of ebullient youth. In fact, the entire theory of conflict as applied to functional mental disorder is constantly buttressed by case histories in which intra-individual conflict is presented as an outgrowth of interpersonal conflict having its roots in sibling rivalry, parental rejection, overprotection in childhood, too much or too little discipline, overly rigid parental standards, and many other deviations from the mental hygienist's ideal of wholesome parent-child relationships. These relationships are commonly taken as the core of the individual's

inner life or fundamental *Erlebnisse* which steer his subsequent psychological development.

Neuroses as Social Disorders.—It is not difficult to understand why, in the light of these considerations, a psychiatrist like Gillespie (1942, pp. 36-39) concluded that "psychiatry becomes a social science" as a result of trying to fathom the nature of neurotic behavior. In fact, he classified neuroses as "social disorders of individuals" and maintained that "the essential pathology of psychoneurotic reactions is a social psychopathology."

Whether psychoanalytically oriented psychopathologists would endorse an opinion like Gillespies' is not to be answered too hastily. Karen Horney (1936), of course, leaves no doubt about her recognition of the crucial role of social factors in shaping neurotic personalities. Even so orthodox a Freudian as Fenichel (1945, p. 464) is probably also to be classified as sympathetic with this viewpoint, for in his last book he stated explicitly that "the character of man is socially determined" and that "the inconsistency of the modern neurotic personality corresponds to the inconsistency of present-day education."

However, Franz Alexander (1942, p. 205), another Freudian or neo-Freudian, introduces a dissident voice. He seems to place more stress on impulsive factors by regarding the latter as primary and cultural stimuli as secondary. In his own words: "Cultural conditions may encourage and strengthen one attitude rather than another, but culture does not introduce anything into the organism which is not already there." Furthermore, on a later page (op. cit., p. 241) he points out that factors of culture are not of "primary importance" for the psychiatrist. But such an opinion is rather exceptional among contemporary students of functional disorder. Nor is it altogether consonant with some views expressed by Alexander (1935) on other aspects of character anomalies.

The vacillation or uncertainty which this suggests may be a reflection of the troublesome distinction Freud made between the "actual neuroses" and the psychoneuroses. By the former term he was referring to the role of toxic, metabolic, and constitutional factors in the production of functional disturbances, while the latter

term was reserved for functional disturbances due to those experiential factors involved in the symbolism of dreams and other manifestations of unconscious conflict. To a limited extent the distinction in question is an outgrowth of the teachings of late nineteenth century psychopathologists who attributed hysterical behavior to a neuropathic inheritance, thus making somatic deficiencies partially responsible for inadequate personality integration. In other words, the actuality of Freud's "actual neuroses" has to do with such somatic determinations.

By elaborating the dynamic implications of his correlative concept of the "psychoneuroses" and laying the foundation for a theoretic explanatory framework Freud may be said to have bridged the gap between the neurological schematizing of the age of Charcot and the emphasis on cultural factors brought into ever greater prominence in recent decades. In fact, with the recognition of what Freud called superego influences and his acceptance of the relevance for psychopathology of anthropological concepts like totem and taboo, the importance of the social sciences for an understanding of functional disorders waxed as talk about neuropathic factors waned. Students of anthropology and sociology gave more and more support to this new orientation.

Biosocial Orientation. —Within the field of psychology itself there was a parallel shift of emphasis away from dependence on inborn equipment in the way of reflexes and instincts and toward greater reliance on learning, environmental influences, social facilitation, and kindred nongenetic factors. The biophysical outlook of the founding fathers of experimental psychology was being supplemented and in some instances replaced by the biosocial outlook of a later generation of psychologists.

Nor was this gravitation toward the biosocial patterning of psychology devoid of philosophic support. John Dewey (1922), for example, helped to undermine confidence in the instinct hypothesis by stressing the part played by social institutions in shaping many of the characteristics of human nature earlier psychologists had attributed to the relentless imperiousness of the driving force of instinct. Possibly even to a greater extent than Dewey the philosopher G. H.

Mead (1934) gave impetus to this reorientation by his penetrating analyses of the importance of role-taking in personality growth.

The fruitfulness of the concept of role-taking for an understanding of many problems in abnormal psychology is especially exemplified in the recent volume by Cameron (1947). The general import of all these twentieth century developments may be summed up by saying that both general psychology as well as abnormal psychology have become increasingly socialized as professional preoccupation with sensori-motor reactions gave way to concern with broader problems of interpersonal reactions as functions of culturally determined role-taking behavior.

Role-taking and Identification.—As applied to psychopathology, role-taking seems at first to supply the needed key to unlock the doors barring the way to an understanding of the functional disorders. The postural immobility, helplessness, and loss of sphincter control of the catatonic is explained as regressive behavior incident to playing an infantile role. The victim of paranoia with his ideas of reference and delusions of persecution may be portrayed as a man acting as if he were afraid that outsiders might accuse him of harboring perverse intentions or desires. Many clinicians work on the assumption that this fear is a secret or unconscious fear of latent homosexual trends. Another common instance of the application of the concept of role-taking is the obvious one of assuming that the transvestite is playing the role of a member of the opposite sex. There is no need to multiply examples. What is more important in the present context is to consider some of the theoretic implications of these efforts to find the psychological significance of psychopathic behavior.

Role-taking obviously involves identification of some sort at least in the sense that an actor has to identify himself with the character he is endeavoring to portray. Should the actor become so enamoured of his role as to make it his own off the stage as well, then he will no longer be conscious of *playing* a role, for he will be *living* it. In Jungian language his *persona* will have become his *anima*. This is a reminder of the root meaning of the word *personality* and its origins in the role playing of actors. To understand the

personality make-up of his patients the psychiatrist may thus be following a very promising lead when he tries to find out what identifications, both positive and negative, have influenced their lives and what they take to be their roles in the drama of everyday living. This is not too far removed from Alfred Adler's emphasis on the "style of life" as a clue to personality diagnosis. In their efforts to understand neurotic behavior his followers continue to follow his lead of searching for the way in which the patient is coping with the need to play the role of a successful or superior person. Different types of personality are supposed to develop depending on the way in which victims of real or imagined inferiority strive to compensate for their feelings of inadequacy or insecurity by identifying with real or fictional heroes

Typologies and Role-Taking.—This psychiatric emphasis on role-taking seems to have resulted in the multiplication of typologies. Thus the role of thinker or dreamer becomes the introvert type or the role of miser or pedant becomes the anal type As the latter reference to pedantry suggests, Freudian concepts have been especially influential in giving rise to typologies of one sort or another.

In the sense that Freud's theorizing took its point of departure from clinical observation of neurotic behavior and the results were then applied to normal behavior, the psychoanalyst's typology may be viewed as another example of the way in which abnormal psychology may affect normal psychology. Contemporary references to people as oral dependent types or oral aggressive types are reminders of the way in which Freud organized a characterology as a derivative of his libido theory.[7] This theory, it will be recalled, was a product of Freud's acceptance of the soundness of the instinct hypothesis. For him Eros and Thanatos symbolized the organism's biologically inherent hungers and lusts and fears and angers and desires expressive of basic drives having to do with sex and the preservation of life.

[7] Whether adherence to this theory continues to be a *sine qua non* of psychoanalytic thinking is open to question. Dr. Judd Marmor, a practicing analyst, has been kind enough to call our attention to the article by Ernest Jones (1946) in which acceptance of the libido theory is not specified as one of "the essential characteristics of psychoanalysis."

Erich Fromm (1947, pp. 38-117) has found reason to object to this Freudian "mechanistic-naturalistic explanation" of the origin of character types. As he sees it, Freud was dominated by the biological concepts prevalent at the turn of the century. This meant that Freud conceived of the organism's energy system to be a "substantial not a relational entity." Traits of character were then seen as resultants of intra-individual wrestlings with the energy of the instincts by means of mental mechanisms like sublimation and reaction formation. Twentieth century "progress of psychoanalytic theory," Fromm maintains, gave rise to a different orientation "in line with the progress of the natural and social sciences."

This orientation broke away from the early Freudian view of the individual as an *isolated energy* system. Instead it stressed the relationship of the individual to himself, to other people, and to his surrounding world. Energy release was seen as governed by such relationships. As Fromm also reminds us, it was because of this new orientation that one of its pioneer sponsors, Harry Stack Sullivan, ventured to suggest that psychoanalysis be defined as a "study of interpersonal relations." The social science connotations of this definition should be obvious. They become even more obvious in the light of Fromm's neo-Freudian typology based on the implications of this definition.

Unlike many Freudians, Fromm does not derive his descriptions of personality types from ingestive and eliminative functions, but from the vocabulary of man's economic behavior. He writes about productive and nonproductive people as well as about those who are predominantly receiving or accepting. He sees others as more exploitative or taking in their behavior and still others as governed by hoarding or preserving tendencies. A fourth type is seen as manifesting what Fromm calls the marketing or exchanging orientation.

There are, of course, additional elaborations of the system and many of the suggestions are both novel and stimulating, but their evaluation would constitute too much of a digression. For present purposes it is enough to note the shift in basic viewpoint underlying Fromm's typology so that the social implications of Freudian psycho-

dynamics are made the key to personality evaluation. As Holt (1915) did many years ago, Fromm thus ventures to work out a psychoanalytically rooted psychology of ethics. Quite manifestly this constitutes an excursion into value theory and to this extent represents a departure from the purely biological emphasis of Freud's own work.

Erfahrung vs. Erlebnis.--Typological psychology is often involved in questions of value either explicitly or implicitly. It is especially germane to the present discussion to call attention to the work of Jaspers (1913) in psychopathology. He was very much influenced by Spranger's efforts to develop a psychology of understanding. It will be remembered that Spranger, taking his point of departure from Dilthey's dissatisfaction with the natural science emphasis of nineteenth century psychology, developed an approach to psychology which stressed the concept of personal value as crucial for an understanding of human behavior. It is this approach that Jaspers has used in his efforts to understand abnormal human behavior.

The result is a very different kind of discussion of mental disorder from that associated with the Kraepelinian tradition. In some respects the difference may be compared to the difference between human nature as described in the conventional textbook of psychology and human nature as portrayed in a psychological novel. Kraepelin's psychopathology was essentially descriptive. Jaspers recognized the place of such descriptive efforts, but regarded them as inadequate to supply an understanding of the morbid personality.

He proposed that a distinction be made between "causal connections" and "understanding connections." The former is a term belonging to the realm of the natural sciences, whereas the latter is unique to the *geisteswissenschaftliche* orientation. The difference in meaning can be illustrated by reference to two possible "scientific" accounts of starvation. One that stresses "causal connections" will have much to say about gastric contractions, dehydration, atrophy of tissue, and changes in blood chemistry. It will be an *impersonal* account. The other, concerned with "understanding connections," will have much to say about the victim's fear of death, worry over

his family's welfare, longing to see his children once more, recurrent hopes of sudden rescue, frantic search for something edible, and kindred themes. The account will be highly personalized and close to the immediate realities of starvation as they are experienced by the victim himself.

In some respects the distinction involved in the two accounts reflects the distinction between knowledge-about and knowledge of acquaintance. It also reflects the difference in connotation between the two German terms for experience: *Erfahrung* and *Erlebnis*, respectively. The natural science type of psychology is more concerned with *Erfahrung* and the kind sponsored by Jaspers with *Erlebnis*.

Diagnostic Quandaries.—In terms of methodology the task of establishing techniques for validating *Erlebnisse* and working out their laws has been and continues to be a subject of controversy. As applied to abnormal psychology the controversy has involved debates regarding the scientific respectability of clinical methods as opposed to laboratory methods. The clinician is likely to do his professional thinking in terms of diagnostic categories. He pigeonholes his patients as being manic, or schizophrenic, or oral aggressive.

The pigeonholing varies with changing nosological fashions and the particular psychiatric system to which a given clinician lends his allegiance. Neurasthenia was very common in the days of S. Weir Mitchell but is much less common today. Similarly, a few decades ago Janet's concept of psychasthenia was introduced much more frequently than in recent years.

There are contemporary diagnosticians who never "see" any of their patients as neurasthenic or psychasthenic. Does this mean that the syndromes in question never existed or that, if they did exist, they were unique to their times and tended to disappear as precipitating environmental stresses ceased to operate in the lives of later generations? Or does it mean that the syndromes are still with us except that they are subsumed under different diagnostic labels? It is not easy to give confident and certain answers to questions like these, for different specialists will answer them differently.

What is of more immediate importance is to consider the psychological implications of the recurrent shifts in diagnostic schemata.

Such shifts may be more indicative of changes in the diagnosing observer than in the clinical facts to be observed. The situation is comparable to the vagaries of type psychology as exemplified both at the popular level in everyday life and in the more technical typological literature. So-called personality types reflect diagnostic categories or conceptual schemes whose one-sided simplicity does violence to the complexity of personality organization.[8]

All sorts of typological ghosts haunt the psychological graveyard. Hippocrates introduced apopleptic and phthisical types while Galen pinned his faith to humoral types in the doctrine of the four temperaments. Modalities of imagery became focal in Galton's analysis only to give way some decades later to eidetic imagery in the system sponsored by Jaensch. Men like Kretschmer, Viola, Naccarati, Draper, Bauer, and Sheldon followed constitutional clues in the search for psychodiagnostic techniques.

In all these divergent approaches one common factor seems to be the notion that preponderance of some feature, trait, organ system, or other variable can be singled out as a central reference point for personality classification. Furthermore, if the preponderant factor be viewed as *abnormally* salient, then the *abnormal* becomes the guiding clue for the typologist. Under the circumstances the resulting typologies are not likely to possess more evidential value than their antecedent theoretic presuppositions.[9]

Diagnosis and the Psychologist's Fallacy.—Psychodiagnosis is not unrelated to topological thinking. Because of what was just referred to as a possible weakness in the latter kind of thinking an insidious form of the psychologist's fallacy is apt to obtrude itself in many psychodiagnostic procedures. This occurs whenever a diagnostician reads into a given case what the typological or diagnostic

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[8] Kanner (1944, p. 707) refers to such schemes as "monopolistic theories" in contrast to the "pluralistic considerations" characteristic of Adolf Meyer's psychobiological approach.

[9] This is not intended to rule out the possibility of a valid and useful typology being developed eventually. As Chein (1943) has brought out, such a typology ought to be genotypical rather than phenotypical in its emphasis. This means a typology based on motivation more than on behavior and, as elaborated by Chein, one that is more nomothetic than idiographic.

category demands instead of restricting his judgment to what the patient or client reveals. The methodological blunder involved is in line with Allport's trenchant observation to the effect that man possesses traits but fits a type.

Within certain limits some currently fashionable projective techniques exemplify this kind of error, for there is a frequently overlooked tendency to interpret test data by fitting them into personality types the clinician associates with a given technique. In the Rorschach test,[10] to cite a convenient instance, it might well be asked whether the projection is limited to the patient's utilization of the blot material or whether the clinician's subsequent interpretation may not constitute a secondary projection. This is the equivalent of saying that some Rorschach interpretations may be more expressive of the uniqueness of the clinician's outlook than a dependable account of the patient's personality characteristics. Terms like coarctated, extratensive, and preschizophrenic may sometimes reflect what is attributed to a patient rather than what is exhibited by the patient. Whenever this happens, the examiner is projecting and the alleged objectivity of the test is weakened irrespective of the scoring refinements presumably underlying the examiner's conclusions.

Abnormal Psychology and the Egocentric Predicament.—In a friendly, impressionistic account of the differences between American and British psychology Stephenson (1948) recently called attention to the fact that in England the psychologists continue to be seriously concerned with the historical roots and philosophical implications of current concepts and theories. In his opinion American psychology "could do with at least a flavour and seasoning of this same philosophical interest." Following this suggestion it may not be amiss to hint at a persistent philosophical problem implicit in what was just said regarding projective and other contemporary psychodiagnostic techniques.

As employed by the clinical psychologist—and his work may be viewed as applied abnormal psychology—these techniques are intended to shed light on the mental quirks of other people. To the

[10] For a recent appraisal of the methodological status of this test consult the article by Thurstone (1948).

extent that they are concerned with motivational dynamics, in contrast to descriptive symptomatology, they have something to do with the way in which clients or patients come to grips with problems as they are experienced. In this sense the psychological examiner is venturing to utilize a psychology of experience even though he may be doing so at an unverbalized level. A little reflection will show that the term experience used in this context is experience as *Erlebnis* and not experience as *Erfahrung*. From the standpoint of philosophy this meaning of the concept of experience cannot be divorced from the egocentric predicament.

The relationship between *Erlebnisse* and the egocentric predicament can be clarified by reviving a traditional illustration of the difficulty of reconstructing or understanding the experiences of another person. According to this illustration, Peter can never understand Paul in 100 per cent complete fashion because, despite the most strenuous empathic efforts and the accumulation of imposing test data, Peter can never relinquish the last vestiges of his own outlook as he tries to view the world as Paul sees it. To accomplish this he would have to *be* Paul. At best he can only approximate an understanding of Paul's *Erlebnisse*. And the magnitude of the discrepancy between this approximation and the real Paul cannot be measured. Presumably the discrepancy is not as large when Peter is an American psychologist studying an American Paul belonging to his own class and caste. It is somewhat larger if Paul is a product of a radically different cultural milieu. Should Paul be seriously disturbed mentally, the discrepancy may be very, very large and this may account for the clash of opinion as different mental specialists, unmindful of the bearing of the egocentric predicament on their psychological quest, proceed to "explain" Paul's abnormalities.

Norman Cameron (1944, p. 903) has called attention to a circumscribed variant of the distorting effect of failure either to allow for or to circumvent the egocentric predicament. He reports that his schizophrenic patients, confronted with the materials from the Vigotsky sorting test, sorted in terms of such categories as weight, color, shape, and some others. The categories employed did not fit the conditions laid down by the test administrator. Results of this

kind have induced other clinical psychologists to infer that schizophrenia involves impairment of what Kurt Goldstein has called the abstract attitude. By this they mean, among other things, that the schizophrenic cannot generalize or bring appropriate categories to bear on problem situations. In the light of his experimental results Cameron regards an inference or interpretation of this kind as unwarranted. In his own words it amounts to telling the patient, "Unless you think of this problem the way I have learned to, you are not generalizing at all."

A Neglected Aspect of Abnormal Psychology.—As was just implied, Cameron is not willing to overlook the *Erlebnis* aspect of abnormal psychology This aspect of the field, it seems to us, has been somewhat neglected. In other words, preoccupation with matters of symptomatology, clinical testing, psychodynamics, and psychotherapy has tended to shunt out due consideration of the fruitfulness of a more direct study of the experiential bases of disordered mental life, by systematic analyses of personal accounts of abnormal experiences. No comprehensive collection of such accounts is available in English. However, Karl Birnbaum (1926) paved the way for a contribution of this kind in his German collection published almost thirty years ago. His volume of "pathological documents" is still a rich storehouse of primary data for the student of abnormal psychology.

The twenty-seven chapters of the book deal with a wide range of abnormal phenomena as experienced and reported by men like Johannes Müller, Walter Scott, Goethe, Swedenborg, Zola, Fechner, Robert Schumann, and a long list of others. Many of the matters brought to light are not even mentioned in the average textbook. For example, the fact that Schiller kept rotting apples in his desk because their smell facilitated his creative work is not generally known, nor is it easy to explain the phenomenon in the light of conventional accounts of creative thinking or of abnormal cognition. Birnbaum's work accomplished for abnormal psychology what Starbuck and James did for the psychology of religion early in the present century and what Gordon Allport has more recently advocated in the general area of personality theory.

The Quest for Standards and System.—By this time it should be clear that the multiple facets of abnormal psychology render it difficult to subsume them under a single comprehensive theory. One might even venture to defend the thesis that implicitly there can be as many systems of abnormal psychology as there are distinctive systems of general or normal psychology. Without elaborating this contention it suffices to hint at the kind of evidence to be marshalled in its support. It is enough to recall the way in which McDougall organized the field of abnormal psychology in terms of the hormic principle or the way in which H. L. Hollingworth worked out a different organization of the same field in terms of his redintegrative principle. To seek a definitive system of abnormal psychology may consequently be as fatuous a quest as seeking a definitive system of general psychology.

It was pointed out at the beginning of this chapter that the term *abnormality* has not been employed in rigorously consistent fashion, and so both the nature and scope of abnormal psychology have varied with the perspective of writers for whom the concept of abnormality has varying connotations. In recent decades, however, there seems to have been growing acceptance of the statistical as opposed to the dichotomous interpretation of abnormality. This, as has already been indicated, brings abnormality and normality within the same frame of reference: the issues are reducible to matters of degree and not of kind. In the light of the dichotomous approach one is justified in asking whether a given person is sane or insane, or normal or abnormal. The statistical approach rules out the justification for such disjunctive questions. Instead one is required to think in terms of degrees of variation along a continuum of change from normality to abnormality.

Some of the difficulties inherent in an unqualified statistical approach have already been stressed. There is no need to review all of them at this point. For present purposes it is sufficient to be reminded of the difficulty of reconciling all of the implications of the adjustive approach with those of the statistical. Normality as sound adjustment becomes a slippery concept in the light of a view which makes the statistical reaction average of each community the

standard of measurement. This is the view which implies as many standards of normality as there are distinctively different patterns of culture. It is the view which, to repeat Glover's illuminating phrase, makes it possible for the sane of one culture to be the insane of another. It is a view which seems to make talk about *universal* standards of normality just idle talk. This follows because the view in question developed out of the implications of the doctrine of cultural relativity.

Is there any way of reconciling the conflict between a quest for universal standards of normality and the plurality of such standards implicit in the doctrine of cultural relativity? This question was anticipated in an earlier section when homeostasis was mentioned in connection with routine medical diagnoses. Homeostatic factors, it was pointed out, are employed by the physician in his clinical determination of abnormalities of biochemical function. The *universal* applicability of these criteria to all ethnic groups and social strata, it was suggested, makes it possible for the medical man to recognize disease in any part of the world. It was also pointed out that this holds true for all medical specialties with the exception of psychiatry. Functional disorder conceived of as a product of culturally determined conflict looms up as the stumbling block. Diversity of culture, so the argument ran, renders it impossible to specify psychiatric criteria of normality analogous to the homeostatic criteria of general medicine.

Is there no possibility of overcoming the consequent parochialism of psychiatry? In a century in which social science fields are being more assiduously cultivated than ever before—a century which some have called the "century of the common man"—it is appropriate to ask whether such parochialism does not constitute an anachronism. Why should there be two sets of criteria for appraising the health of man: a universal set when he is being viewed as a biological organism and a group of relative ones when he is being considered as a person? Why should not social scientists, those concerned with studying man as a product of his culture, consider the possibility of determining essential minima of what every society must provide to safeguard the mental welfare of its members? If

the four freedoms are recognized as prerequisite for emotional security, are they to be thought of ethnocentrically as essential only for people like us or are they relevant to the needs of *all* human beings?

Working out the answers to the foregoing questions is not germane to the scope of the present chapter. They would entail issues going far beyond the limits of the restricted field of abnormal psychology. The sole justification for introducing them in the present context is to suggest that once the social scientists can work out the mental hygiene equivalent of the physiologist's homeostatic factors, then it will be possible to emancipate the psychiatrist from his bondage to the dogma of cultural relativity and, like his colleagues in ophthalmology or surgery, he will be free to practice his specialty among any people in any part of the world. When this time comes the abnormal psychologist will also have a better understanding of what he means by an abnormal personality

The Concept of Psychotherapy

By way of rounding out our quest for theoretic issues embedded in the field of abnormal psychology it will be helpful to consider the topic of psychotherapy. As in general medicine, psychological treatment may be based on a crude empiricism or on a more or less plausible theoretic foundation. Hardly any space will be devoted to the former types of psychotherapy, since only the latter are germane to our purpose Stated otherwise, only those therapeutic schemes will be considered whose rationale is explicable in terms of some theory of the abnormality they are presumed to correct.

Preliminary Survey.—An almost bewildering array of modes of psychotherapy has been proposed and is being used by contemporary practitioners. These include individual as well as group therapy, directive and nondirective procedures, brief psychoanalysis as well as the traditional kind lasting months or years. In addition, one might list play therapy, will-therapy, occupational therapy, psychodrama, bibliotherapy, hypnotherapy, narcosynthesis, relationship therapy, waking suggestions, distributive analysis, dynamic growth therapy, conditioned response methods, and various so-called re-

educative schemes. There is no need to consider all of these in detail nor to stress the fact that, despite the variation in terminology, there is much overlap among many of them. However, the very multiplicity of proposals points to divergencies in practice as well as divergencies in theory.

Unless the clinician has a therapeutic plan, his procedure is apt to be an *ad hoc* response to symptoms or a product of impulsive improvisation. Planful procedure calls for adequate conceptualization of the nature of the problem to be solved. Such conceptualization must obviously imply endorsement of some theory of behavior disorder; hence the reasonableness of searching for theories of abnormal behavior by examining the plans or techniques proposed by the psychotherapist.

The Problem of Control. Viewed broadly, without affiliation with any special school of psychotherapy, it might be said that mental patients have lost or lack control of reactions ordinarily controllable by the normal individual. Everyday notions of the nature of insanity reflect this emphasis on loss of control. It is also reflected in those statutes which make the irresistible impulse test a tenable addition to the traditional M'Naghten Rules defining the legal aspects of insanity.

In general, mental disorder concerns the courts because its victims are sometimes a menace to themselves or others. The ament cannot control his behavior due to deficient learning capacity, while the psychotic exhibits loss of control incident to pathological involvement of perceptual, imaginal, affective, and other psychological processes. Even the less seriously disturbed conditions classified as neuroses may also be regarded as involving disruptions of control. Fenichel (1945, p. 19) had reference to this when he wrote that "the common denominator of all neurotic phenomena is an insufficiency of the normal control apparatus."

In some respects this formulation may be interpreted as an outgrowth of Freud's recognition of the central role of anxiety in the neurotic's difficulties. At one time he regarded anxiety as a product of the patient's fear of losing control of repressed impulses. At a later stage in the development of his system he made anxiety the

cause rather than the effect of repression. Anticipation of the dire consequences likely to follow from indulgence of a questionable impulse arouses anxiety and this brings about repression of the impulse. This reversal of the earlier formulation thus makes anxiety the antecedent and repression the consequent. What is important for present purposes is the fact that in both formulations Freud was concerned with the neurotic's efforts to achieve control over incompatible desires or impulses of hazardous intensity. Under the circumstances the task of the psychotherapist might be construed as that of aiding his patient to learn more efficient techniques of self-control. Viewed in this light psychotherapy is seen as a form of teaching. If this is sound, then rival schools of psychotherapy are comparable to divergent systems of education. This means that the center of gravity of psychotherapy ought to be shifted from the biochemical orientation of general medicine to the frame of reference within which the psychologist discusses learning, habit formation, motivation, attitude formation, and kindred themes.

Therapy as Teaching. In some respects a term like psychotherapy may be misleading. The suffix connotes modes of treatment of a medical nature like massage, hot packs, drugs, and diet. Therapy of this sort is not germane to the behavior disorders. The word itself has to do with the art of healing and its retention in the form of *psychotherapy* inevitably keeps the notion of healing salient in the thinking of the psychotherapist. This is all to the good provided one is dealing with a form of sickness in the traditional meaning of sickness. But if what is being dealt with does not conform to the latter meaning, then thinking of oneself as a healer may militate against relevant consideration of what one ought to do in behalf of the troubled or muddled patient.

This nonmedical approach to neurotic behavior was already implicit in Freud's own thinking. His acceptance of the idea of having people who were not physicians serving as psychoanalysts suggests belief on his part of the dispensability of a formal medical background for understanding the nature of functional disorders. This in itself lends support to the idea of questioning the wisdom of using a word like *therapy* to designate both what a medical man

does for a malaria patient and what another practitioner does for a timid adolescent. In the interest of clear thinking it will be advisable either to enlarge the scope of the meaning of therapy or to remind ourselves that teaching functions are not restricted to formal classroom procedures but include any means of enabling a person to change his outlook, attitudes, level of aspiration, frustration tolerance, degree of emotional control or whatever else may have a bearing on personal efficiency.

One reason for stressing the desirability of envisaging psychotherapy within the framework of teaching functions is for the purpose of shunting out remnants of esoteric overbeliefs still aroused by a word like therapy. In part such overbeliefs hark back to mysteries associated with hypnosis as a form of therapy. In part they derive from still earlier, pre-scientific, occult maneuvers incident to exorcising demons, warding off evil spells, and effecting cures by magic potions, the laying on of hands, or a shaman's incantations. Current phrases like being "in analysis" or "in therapy" tend to suggest that the client or patient is undergoing something mysteriously different from the routine business of achieving more effective ways of disposing of personal problems. Unless the therapist has considered the nature of his role in critical fashion he too may succumb to the temptation of believing himself to be engaged in a mysterious venture involving unique powers and self-aggrandizing talents. To think of himself as a teacher may be less flattering to his ego than to regard himself as a therapist. Nevertheless the former designation is more likely to facilitate realistic consideration of his functions than the latter.

At the risk of a slight digression some implications for clinical psychology of the reorientation under consideration might be pointed out. Not so very many years ago the place of the psychologist in the neuropsychiatric team was ancillary or subordinate in the sense that his duties were limited to test administration. He was not entrusted with responsibility for the patient's therapy. This was regarded as a more distinctively medical function. Such an allocation of duties is justified provided functional disorder is viewed as a sickness. However, if such disorder is seen as a product of warped attitudes, poor habits of self-control, and similar manifestations of

inadequate learning, then the more recent trend of permitting clinical psychologists to undertake responsibility for treating a patient may be interpreted not as an encroachment on medical prerogatives, but as a legitimate application of methods and principles particularly germane to the field of psychology in the sense that problems of learning and habit formation are psychological rather than medical problems.

The Learning Process and Therapy.—Consideration of psychotherapy as more psychological than medical in nature involves a change in perspective by making understanding of abnormal behavior less a matter of organic pathology and more a matter of the dynamics of learning. A perspective of this kind brings abnormal psychology and learning theory together. It also serves to account for much of the seeming contradiction and confusion among psychotherapists and for the variety of therapeutic techniques being advocated. It can do this because the practical consequences of learning theory influence educational practices. The fact that people are more familiar with the latter will render elaboration of the parallel between educational and therapeutic practices helpful at this point.

Among educational theorists there are those who prefer the authoritarian leadership of the expert teacher directing the efforts of the pupils, giving them assignments, telling them what to do and how to do it. Such a teacher may scold or encourage, reason and persuade, order or cajole, depending on what he believes will produce the educational result he is trying to achieve. Similarly, those who advocate directive psychotherapy see the function of the therapist as somewhat akin to that of an athletic coach. A directive therapist acts as if he knows what the patient ought to do in order to become less aggressive or less timid or less insecure. He may venture to plan the patient's daily regime in terms of work, play, and rest. He may supply factual information when, in his opinion, the maladjustment is attributable to erroneous beliefs or a misinterpretation of something the patient has heard or read. On occasion he may suggest a book or article by way of changing maladaptive attitudes due to ignorance or misinformation. Labeling the latter procedure *bibliotherapy* does not alter its essentially educational character. Nor does

a designation like directive therapy transform any of these techniques into something mysteriously different from what countless parents, teachers, and coaches do in their daily educational work.

In so-called child-centered homes and schools the educational atmosphere is more permissive. An atmosphere of this kind is deliberately fostered by those who regard all learning as self-learning. They react against an outmoded view which sees the learner as the passive recipient of a flood of advice and instruction. Instead they regard learning as an active, goal-directed process in which what the learner does is more decisive than what the teacher says. Moreover, they recognize the importance of respecting goals and aspirations the learner may set for himself. To make a genuine difference in his life these goals must be *his* goals—not those of parent or teacher. Only as he sees goals as congruent with his needs will the process of learning be effective. In brief, this kind of educational theorizing tends to reject authoritarian techniques because they seem to foist extraneous standards and ways of doing things on a helpless or cowed or resistant individual. The goal of the educator, these theorists maintain, should be the development of self-reliant, mature individuals capable of working out solutions to the problems of living with a minimum of dependence on outsiders. And the way to reach this goal, they hold, is to place the responsibility for learning on the shoulders of the learner by encouraging him to do his own seeking, manipulating, and exploring all along the line from infancy to maturity.

What has come to be called nondirective psychotherapy can now be understood as an application of the foregoing educational philosophy to problems of adjustment. The nondirective therapist prides himself on being permissive, accepting, and nonjudgmental, and in seeing to it that his patient or client gets at his difficulties in his own way. Like the teacher in a progressive school such a therapist spurns techniques of coercion, domination, or regimentation. For him the focus of "therapy" lies not in himself but in the patient. He prefers to say little and for the patient to say much. As he structures the therapeutic situation, it will be up to the patient to learn for himself by doing for himself as he wins understanding of himself and gains in self-confidence.

Whether therapy should be individual therapy or group therapy can also be considered as a question having to do with learning theory. It is the counterpart of the educational choice between private instruction on the one hand or participation in group study on the other. The fact that so much of what is called therapy has to do with emotionalized attitudes and the life of feeling and impulse in general does not make recognition and transformation of attitudes, feelings, and impulses uniquely different from changes ordinarily ascribed to learning in other contexts.[11] Whether the changes occur in the privacy of the consulting room or in the more public setting of a group meeting, whether the therapist is directive or nondirective, or even whether he has recourse to books or play are more an indication of the variability and flexibility of teaching methods than evidence in support of a notion which, from the viewpoint of psychology, would regard psychotherapy as involving factors alien to or different from those falling within the scope of learning theory.

There are some additional parallels between educational views and those of psychotherapy to be traced. For example, just as in one sense it is correct to say that a student's education is never finished, so one might say that no patient is ever *completely* psycho-

[11] These changes are not 'uniquely different' in the sense that they have to do with learning. They may, of course, be different from the nonemotionalized learning of school subjects, telephone numbers, acts of skill and the material of content subjects. This brings up the question of the need for a dichotomous learning theory implicit in many discussions and rendered explicit by Tuttle (1948, pp. 131-137). In fact, Tuttle refers to 'two kinds of learning' and labels one kind intellectual and the other affective. The former has to do with concept formation, insight, and the acquisition of skill or, in general, education in the formal sense. The latter, affective learning, has to do with the cultivation of taste, the emergence of preferences, aversions, prejudices, and, in general, what is ordinarily subsumed under the phrase emotional conditioning. A simple way to clarify the distinction is to note the difference between the task of a music teacher trying to teach a child to play the violin as opposed to his task of trying to get the child to like one kind of music rather than another. Learning to read music, Tuttle would say is intellectual learning while learning to prefer Bach to Mozart is affective learning.

Much of psychotherapy calls for the utilization of principles of affective learning as these come to be worked out. The common observation that "mere intellectual" analysis of a personal problem is not sufficient to dispose of its neurotic roots may be an oblique reference to the need for affective learning if the therapy is to be successful. In terms of psychoanalytic technique this means "working through" the problem by rearousal of the emotional attitudes associated with it.

analyzed. More learning or deepened insight is always possible As insight is gained attitudes change and to the extent that attitudes govern behavior just to that extent may behavior change.

It may be illuminating to consider a psychoanalyst as playing the role of educator or teacher with the proviso that the subject to be studied is the personality of the patient. One might ask what might happen if a student could meet with his favorite professor for an hour a day over a period of four years just to talk about himself—his loves and hates, his resentments and frustrations, his ideals and aspirations, and anything else he cares to introduce—and to be encouraged to talk without fear or censure or ridicule. Irrespective of such a professor's own views of human nature this kind of learning situation is apt to facilitate modification of the student's own appraisal of himself His personal value system is apt to be modified. What is more, his total philosophy of life may be revised so that within limits his friends might refer to his "changed" personality Under the circumstances at least part of the reported successes of personality transformation attributed to psychoanalytic procedures may be functions of the opportunities for self-learning over a period of years provided by the analytic setting. Even the high cost of such procedures may have something to do with the educational outcome. A would-be golfer paying for an expensive golf lesson is more likely to apply himself than if a generous teacher gives "free" tuition.

Therapy as Detective Work.—In the early history of psychoanalysis great stress was placed on ferreting out the hidden motives responsible for the neurotic's symptoms. It was believed that once the patient gained insight into the dynamic origin of his difficulties the stage would be set for their solution. Bringing these latent motives out into the open was long regarded as the chief purpose of psychoanalytic probing. Freud, it will be recalled, first tried to get at them by means of hypnosis Subsequently he discarded hypnosis in favor of free association and dream interpretation along with clues supplied by verbal slips, blunders, and acts of forgetting.

The implications of these techniques for psychological theory are momentous. They are products of Freud's utilization of such sci-

entific constructs as a dynamic unconscious, psychic determinism, resistance, and repression. Nor should the relevance of the various defense mechanisms be overlooked in the present context. Displacement, rationalization, projection, reaction formation and the rest served to complicate the analyst's detective work by making crucial motives harder to recognize until defensive disguises were stripped away.

It might well be asked how such ingenious Freudian forays into the dynamics of the neurotic's motivational history can be brought into line with what was just said regarding psychotherapy as a function of learning theory. Woodworth (1948, p. 173) has touched on this in his recent survey of psychoanalysis as a school of psychology. He suggests that for Freud "motivation was practically the whole field of psychology." This implies that all behavior, both normal and abnormal, is to be accounted for by almost exclusive reference to motivational factors. The exposure of these factors, especially the unconscious ones, becomes a central analytic objective. Once this objective is attained and the patient succeeds in coming to grips with his unconscious motive, "there is a chance of dealing with it rationally, and so advancing toward a cure."

However, Woodworth points out that coping with a problem *rationally* is more than a matter of motivation. It calls for the capacity to *perceive* significant relations, to *select* appropriate goals and the means of attaining them, to *anticipate* consequences, to *attend* to the relevant and—in brief—to make use of all *cognitive* functions. Woodworth thus extends the concept of psychic determinism beyond the confines of the circumscribed area of motives by maintaining that the patient's "power *to learn*[12] new and better ways of managing his life can be potent causative factors." The role of learning is consequently implicit even in those theories which, upon superficial examination, seem to reduce psychotherapy to the quest for unconscious determinants of abnormal behavior.

Freud's emphasis on unconscious determinants, the imperiousness of libidinal factors, and the distortions introduced by rationalizations and other defense mechanisms has caused some to view psychoanalytic psychology as altogether or preponderantly anti-intel-

[12] Italics not in the original.

lectualistic. Reason is played down and impulsiveness is played up. Man is viewed as driven rather than directed. To understand behavior and to treat neuroses these driving forces must be tracked down and their genetic roots exposed. However, Freud himself may not have endorsed this account of the drift of his psychology. He was not unmindful of the part played by factors of cognitive control. This is indicated by his references to the reality principle, the vigilance of the censor, and the inhibitory role of the superego. Furthermore, what may be designated as his pro-intellectualistic endorsement is reflected, as Alexander (1941) brought out, in his writing that "the voice of the intellect is a soft one, but it does not rest until it has gained a hearing." Seemingly for Freud man's behavior was not *exclusively* a function of the vagaries of motivation and conation.[13]

Dissociation and Unconscious Determinants.—For many psychotherapists, as was just indicated, it is important to track down the unconscious determinants of functional maladjustment. Obsessions, phobias, fugues, and other abnormalities cannot be corrected, these therapists maintain, until one learns what unconscious factors are responsible for them. Crucial theoretic presuppositions are bound up with this familiar standpoint, and no discussion of theory in abnormal psychology would be complete without consideration of such presuppositions.

The theories involved go back to the pioneering ventures of Freud and Janet in the closing decades of the last century and the first few years of the present one. Both men were influenced by their observations of hysterical patients in Charcot's clinic, but worked independently of one another. Both attributed some of the symptomatology to constitutional factors as predisposing influences

[13] Even those clinical psychologists whose interpretations of Freudian psychology are more congruent with the anti-intellectualist viewpoint, often fail to carry out the practical implications of such interpretations in their routine clinical work. In the course of their institutional and hospital activities they habitually have recourse to the Bellvue-Wechsler test of intelligence for the light this test can shed on the mental status of their patients. It is not easy to reconcile this confidence in the test with a systematic disparagement of the efficacy of cognitive controls, for, according to Wechsler (1941, p. 3), "intelligence is the aggregate or global capacity of the individual to act purposefully, *to think rationally* and to deal effectively with his environment." (Italics not in original.)

but devoted their major efforts to accounting for the experiential or precipitating factors. Both recognized the latter factors as not subject to voluntary recall on the part of the patients. Here their agreement ceased.

In general it might be said that Janet's theorizing was less dynamic than Freud's Although both availed themselves of the concept of dissociation, the former was more content to use the concept as a self-sufficient explanation for the hysteric's amnesia and loss of sensory and motor control Freud pushed the inquiry harder by asking what induced the dissociation of some episodes but not of others and what was interfering with the reinstatement of the dissociated ones. In the course of his work he endeavored to answer these questions by adding such auxiliary concepts as repression, resistance, displacement, and a dynamic unconscious to the central concept of dissociation.

In terms of systematic psychology both Freud and Janet were reflecting much of the associationistic psychology of the period. If normal behavior be accounted for by reference to the effective synthesis of a present problem situation with the effects of previous learning, then abnormal behavior can be accounted for, at least in part, by failure of such synthesis. This is brought out by repeated references in the writing of Janet (1901) to the hysteric's "diminution of mental synthesis," and to his "want of mental unity." The normal person effects such a synthesis and achieves such unity.

Integrated or normal behavior can occur only if today's experience can be articulated with the fruits of past experience. If either kind of experience is not functionally accessible, disarticulation or abnormal behavior takes place Such lack of functional accessibility, it should be obvious, means dissociation In hysterical analgesia, for example, the sensation of pain is dissociated from its customary antecedent of a noxious stimulus such as the jab of a pin or the blow of a hammer The somnambulist, to cite another instance, acts as if the normal association between the bed and the relative immobility of the body had been disrupted. In functional paralysis there appears to be a dissociation between an intended movement and its normal execution. In fugue states the dissociation is more global in character, for the patient acts as if the memory matrix

underlying the sense of personal identity were sundered from the focus of his hysterically conditioned ego organization.

A review of the preceding examples of dissociation will show them to be instances of abnormal discontinuity, but not necessarily of what some would regard as the paradox of unconscious mental processes. Accepting the tenability of the concept of dissociation,[14] in other words, does not automatically commit one to endorsement of the concept of a dynamic unconscious. The everyday phenomenon of being unable to recall the title of a familiar book may be said to illustrate dissociation in the simple sense that the normal association between the thought of the volume and its title fails to take place However, this is not to say that the title is actively present in some unconscious psychoneural facsimile of itself. Its potential recall may justify some concept like Semon's engrams, but the engram theory is not a neurological equivalent of Freud's dynamic unconscious. The latter concept can be clarified by reference to its historical origins.

Hypnosis and Unconscious Motivation. Freud, it may be recalled, spent some time observing the work of Bernheim of the Nancy school. In fact, he was sufficiently impressed by what he saw to undertake the translation of Bernheim's book on hypnosis into German. He was especially impressed by the phenomenon of post-hypnotic suggestibility. This supplied a fairly decisive demonstration of behavior instigated by determinants known to the experimenter but unknown to the subject. A successfully executed post-hypnotic suggestion illustrates *both* dissociation and unconscious motivation. The subject's amnesia for the hypnotist's instructions exemplifies the former and his behaving *as if* he nevertheless did remember them exemplifies the latter. The concept of unconscious motivation is thus an *as if* construct justified by the experimental observation

What Ernest Jones later came to call *rationalization* was early recognized as involved in the behavior of the unconsciously motivated subject carrying out a post-hypnotic suggestion If asked why he carried out the act, such a subject would invent some explanation.

[14] More detailed consideration of this concept is to be found in the volume by Miller (1942, pp. 19, 35, 64-65, 227-230) and in the one by Murphy (1947, pp. 435-440).

In fact, his very inability to relate the act to the hypnotist's commands seemed to justify belief in the reality of unconscious motivational factors. The latter were presumed to be the actual instigators of his act so that any reasons he supplied at the conscious level could then be viewed as spurious. Generalizing from these hypnotic demonstrations one might venture to say that self-analysis of motivation is apt to be treacherously misleading because of failure to get at the really effective but unconscious drives to action. A generalization of this kind is commonplace today as a result of the widespread dissemination of psychoanalytic teachings. Dynamically oriented psychiatrists revert to it constantly in their efforts to account for the abnormal behavior of their patients. It is consequently necessary to examine it somewhat more critically in order to understand both its theoretical and practical implications.

To begin with one ought to ask just what is the motive that prompts the execution of a post-hypnotic act even if the subject is unable to get at it introspectively. Suppose the act calls for untying both shoelaces when the experimenter raps the table three times with a pencil. It may also be posited that upon being questioned the subject explains his act in terms of a feeling of tightness or numbness of his feet due to slightly impaired circulation. At all events, by hypothesis, he seems unable to detect any relationship between his act and the experimenter's signal. The "unconscious" aspect of his behavior has to do with this failure.

It is not easy to specify the precise motive or motives in this more or less typical example of post-hypnotic suggestibility. Is it a matter of pleasing the experimenter? Is it a matter of blind obedience to instructions given by a person in whom one has confidence and who is regarded as an authority figure? These interpersonal relations are not to be dismissed too hastily as irrelevant considerations. In fact, as will be brought out later, they are especially relevant for an understanding of the psychotherapeutic relationship.

For the time being it is enough to note the bearing of the experimenter's instructions on the subsequent behavior of the subject. Is it too much to assume the effect of these to be the initiation of a set or determining tendency on the part of the subject? Cer-

tainly one outcome of the Würzburg school controversy was the demonstration of the difficulty or even the impossibility of describing such sets or Aufgaben or determining tendencies in terms of conscious content. By referring to them as impalpable or imageless or *unanschaulich* the members of this school were calling attention to the "unconscious" nature of such dynamic processes For Külpe and his followers these processes, even though not accessible to introspective observation, were nevertheless presumed to play important roles in the regulation of thought and action Like Freud's dynamic unconscious they were necessary constructs justified by the laboratory protocols. If this interpretation be accepted, assimilation of some Freudian concepts into the tradition of academic psychology may be facilitated

Determining Tendencies and Delayed Reactions. Reverting to the main question under consideration, it should now be clear that post-hypnotic suggestibility has to do with determining tendencies. Initiation of the latter, as Ach demonstrated, occurs in the fore-period As applied to the present example this means untying of shoelaces in response to the tapping signal may be regarded as an automatic rather than a volitional reaction to the signal The volitional process was introduced when the instructions were given by the experimenter and accepted by the subject Why the individual is more ready to accept such suggestions in hypnosis than in the normal state is still open to discussion The problem of motivation is bound up with this readiness to accept and believe But the post-hypnotic act itself is not so much a function of underlying motivation as of the volitional set induced by the subject in his acceptance of the task. The later execution of the task is thus characterized not so much by amnesia for the motivation as by amnesia for the instructions.

As a consequence, the phenomenon of post-hypnotic suggestibility may not be as intimately related to the concept of unconscious motivation as has been commonly believed. It may be more accurate to cite it as an instance of unconscious resolution or unconscious determination. Nor should the interpolation of a protracted interval of time between the induced resolution and the performance of the

suggested act be viewed as a special psychological mystery. The time of performance is specified as an integral part of the original *Aufgabe*. In view of Hunter's (1913) demonstration of man's capacity for prolonged delay in responding to a specific stimulus or signal the subject's post-hypnotic responsiveness ought not to be too perplexing. It can readily be classified as a variant of the general concept of a delayed reaction. In the original meaning of this concept the delay referred to the enforced postponement of reaction to a given signal no longer physically present at the instant of reaction. If the correct response took place, it was assumed that some kinesthetic or ideational surrogate of the signal was governing the choice of reaction. In the post-hypnotic situation there is a slight modification of this sequence, for the ideational or verbal account is given first and the actual realization of the anticipated signal occurs later. However, this modification does not appear sufficiently momentous to render the concept of delayed reaction inapplicable to the temporal aspect of post-hypnotic suggestibility.

Pointing out the bearing of determining tendencies and the concept of delayed reaction on the phenomenon of post-hypnotic suggestibility may help to clarify this phenomenon in part. The dissociation as exemplified in the post-hypnotic amnesia is not clarified nor is the subject's willingness to accept the instructions in the first place explained. To say, as White (1941) seems to hold, that hypnosis is a form of role playing in which the subject performs in accordance with what he believes is expected of a hypnotized person may account for the willingness but hardly for the dissociation. Histrionic skill may enable one to simulate amnesia, but it will not be a genuine dissociation such as one finds in hypnosis and in hysteria. This is not to deny the heuristic value of White's stimulating theory, but further elaboration of its implications would be too much of a digression.

Motivation and Hypnotizability.—It is more germane to the broad question of psychotherapy under consideration in this section to limit discussion to the single aspect of White's theory having to do with what is involved in a readiness to play the role White attributes to the hypnotic subject. As was just implied, the motiva-

tional factors responsible for post-hypnotic behavior are probably more a function of this readiness than of what occurs at the moment of implanting a post-hypnotic suggestion. The latter, it was pointed out, is more accurately to be disposed of in the light of volitional processes governing the introduction of special sets or determining tendencies. If this be so, then post-hypnotic suggestibility supplies only indirect evidence in support of the concept of unconscious *motivation*. It has no more evidential value in this respect than any other phenomenon attributable to hypnotic suggestibility.

A variety of factors motivate people to serve as hypnotic subjects: curiosity, desire to cooperate in an experiment, desire for relief from distressing symptoms, and several others. Whether such motives suffice to account for the induction of hypnosis may be questioned very seriously.[15] The manner, bearing, and general personality makeup of the operator must also be taken into account. At all events some operators succeed where others fail.

The particular interpersonal relationship known as *rapport* must not be overlooked. In part this is related to what the operator symbolizes in the eyes of the subject. If the operator is regarded as one having prestige and the status associated with an impressive manner, a professional title and reputation, his suggestions are less likely to arouse resistance, disbelief, and critical countersuggestions. Stated affirmatively, this means the arousal of an attitude of acceptance, belief, trust, and conviction and the concomitant inhibition of the set to weigh evidence and demand proof. The shunting out of the latter kind of set in itself is the equivalent of dissociation. It makes for the kind of "narrowing of the field of consciousness" Janet regarded as characteristic of the "mental state of hystericals."

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[15] This issue is somewhat bound up with the basic problem of the precise neurological or cerebral changes involved in hypnosis. If hypnosis be viewed as related to "trance" states induced in animals or what some have called "animal hypnosis," then the present emphasis on social factors might be receiving too much stress. It may be, though, that the relationship is more superficial and restricted than the term "animal hypnosis" suggests and that the immobility exhibited by animals is the chief kinship with hypnosis as manifested by human beings. If this immobility proves to be like the paralysis of fear, then there would be even more reason to question the suitability of the term. For a stimulating survey of the literature on "animal hypnosis" the article by Gilman and Marcuse (1949) should be consulted.

It is to be noted that trust and confidence in the operator facilitating rapport between him and the subject is the outcome of reciprocal social attitudes governing this sort of interpersonal relationship. In this sense hypnosis impinges on the field of social psychology. The prestige of the operator tends to arouse a dependent orientation toward him on the part of the subject. What kind of identification this involves will vary from subject to subject. Dependent attitudes are apt to be elicited by persons symbolizing authority, expert knowledge, great skill, or strength. Genetically considered, they reflect the attitude of the young child toward its protective parent with all that this connotes in the way of emotional security, naive trust, feelings of being loved and wanted.

Hypnosis may thus involve, as Freud believed, a reactivation, at least within certain limits, of this kind of trusting confidence in a father figure. The motivation of the subject is thus apt to be diffuse and rooted in his conditioned reactions to authority figures and father figures. Unless he has been psychoanalyzed or given some equivalent opportunity for honest self-examination, the average subject is likely to be "unconscious" of this aspect of his implicit motives. In this indirect fashion it might be correct to regard post-hypnotic suggestibility and other hypnotic phenomena as products of unconscious motivation. At all events it is unlikely that the average subject can specify just what the hypnotist, viewed as a social symbol, touches off in the way of dependent, trustful attitudes; hence, his ignorance of the motives back of the role-playing, the determining tendencies, the rapport, and whatever else may be involved in hypnosis.

The Transference Relationship. The kind of psychotherapy developed by Freud grew out of his early experience with hypnosis. He discontinued the actual induction of hypnosis, but did not abandon the practice of having the patient stretch out on a couch. Incidentally, the precise influence of the horizontal body position on the arousal of dependent attitudes and the facilitation of recall still awaits experimental check.

The dominant mental set Freud tried to arouse in the patient had to do with the "basic rule" of giving free expression to every idea

without critical scrutiny of its relevance or importance. Once having *learned* to do this the patient could more readily give vent to strong feelings and describe yearnings and impulses of the most intimate and even "depraved" character. This took place in a social setting since "free association" was the equivalent of confessing things to the analyst or at least of having him listen to the abreacted episodes. Freud was not unmindful of the bearing of this social setting on the therapeutic efficacy of the procedure His emphasis on "transference" was one way of referring to the patient's reaction to the analyst as a social symbol—as a surrogate for a father figure or some other person important in the moulding of the patient's affective patterns

How the analyst reacts to what the patient has to say becomes an integral feature of the total transference relationship The affective patterns can become modified as a result of this relationship For example, should the patient give expression to feelings of burning resentment against a tyrannical father who had punished him severely and often unjustly years before, the analyst's understanding acceptance of this antagonism toward the father might well influence the constellation of factors bound up with the resentment Instead of arousing furious counter-resentment, as would have been the case had the patient talked this way to his real father, there is no sign of disapproval or hostility from the present father surrogate The patient continues to be respected as a person worthy of help, no matter how extreme his self-revelations Feelings of guilt or anxiety previously associated with incipient but repressed upsurges of hatred for his father may become less acute, because the analyst in his role as father-substitute makes no threats, fails to punish, and registers no condemnation for what the patient had been taught to regard as a violation of the fifth commandment All this involves change in the patient's attitudes toward himself for harboring such impulses. Even the symbol of "father" which had elicited such impulses may now elicit some of the kinder impulses rooted in the patient's appreciative gratitude for the help and understanding being received from the father-surrogate

The Role of Conditioning. - Interpreting symbols differently or reacting to them in modified form is, of course, part of the learning

process It is in this sense that much of psychoanalytic therapy is to be brought into line with the psychology of learning By way of clarification it should be noted that the theory of conditioning is applicable to the illustration just introduced Originally the patient had been conditioned to respond to the idea of father with feelings of resentment guilt, and anxiety Reactivation of such feelings in the presence of another father symbol involved a disinhibition of the original inhibition of overt expression of hostile feelings Furthermore the analyst's nonjudgmental acceptance of such expression tends to make continued repression of such feelings less imperative By symbolizing the rôle of friendly nonpunishing father the analyst is the means of bringing different emotional sets to cluster around the patient's conditioned emotional reactions to the father-symbol and those second-order conditionings having to do with reactions to authority, discipline and kindred accompaniments of a *pater potestas* regime of child rearing

Another way of formulating these changes in terms of the theory of conditioning would call for consideration of the specificity of conditioned reaction Students of conditioning will recall that a high degree of specificity of conditioned discrimination is not achieved during the early stages of training The dog in the Pavlovian paradigm responds to a wide range of tones in the beginning of the training and only in the terminal stages is the conditioned salivary reaction restricted to a specific stimulus of for example a tone of 256 cycles Initial reactions are overgeneralized In human learning this is exemplified by the very young child's tendency to call all men daddy then gradually restricting this more and more to men who look something like the actual father and finally reserving the term for the latter alone

What this means is that in learning one reacts not merely to a stimulus *qua* stimulus but also as if that stimulus were a representative of a whole class of stimuli To employ a stock illustration, the first glimpse of an individual Hottentot is regarded as representative of "what Hottentots look like." And for a time all others will look like the first one as revealed by the traveler's report that all Hottentots look alike Only after living among them will the requisite discriminations be made so that the uniqueness of each one will be

noted. The bearing of this familiar process on concept formation, the genesis of group-prejudice, the origin of fixed ideas and warped social attitudes should not be overlooked.

The present reference to conditioning should not be interpreted as more than suggestive of the way in which one kind of learning theory can be applied to problems of psychopathology and psychotherapy. It so happens that many experimental studies in this area, especially those having to do with behavior disorders in animals, took Pavlov's pioneer experiments as a point of departure. The work of Massern an (1943) Gantt (1941) and Liddell (1947) are well known exemplifications of the extension of this Pavlovian approach. In fact, for Liddell the routine conditioning procedure involving the relative immobilization of the animal in a restraining harness is in itself a traumatizing procedure. This is independent of the induction of audiogenic seizures or the use of painful shocks as stimuli. The theory of conditioning has thus given rise to a large body of reports dealing with abnormal behavior; hence its relevance in the present discussion.

By way of illustrating the possibilities in some of these animal studies for dealing with the problem of anxiety it may prove helpful to consider Farber's (1948) work on response fixation. This experimenter studied such fixation under what he regarded as "anxiety and non-anxiety conditions." He trained two groups of rats to run to the goal compartment on the preferred side of a single-unit T-maze. Each animal was given 100 trials during the training series. However, during the first 40 trials the rat was permitted to run to the food without interruption, while during the last 60 trials an electric shock was administered just as the animal came to the critical choice point in the maze. This shock was assumed to arouse the equivalent of severe emotional disturbance in a human being. Being forced to traverse the choice point in order to get food presumably reinforced the turn to the preferred side since the turn in question resulted both in the food reward and the tension reduction incident to relief from pain.

To check on the existence of such response fixation, Farber proceeded to feed one group at the choice point. This served to change the cue significance of this point for the animals from an

exclusive danger signal to one that elicited expectation of reward as well. On the day following this feeding the problem was modified for *all* of the animals by shifting the locus of the goal box to the nonpreferred side of the maze. Neither shock nor food was introduced at the choice point from now on. Both groups of rats were made to run the maze until the original response to the preferred side had suffered experimental extinction.

The results showed a significantly greater number of trials necessary to achieve such extinction for the group that had not been fed at the choice point. The other group, the one that had been fed at the choice point, proved more flexible or adaptive. Maladaptive fixation was present in the former group because of the absence of special tension-reducing experience. In somewhat anthropomorphic terms one might say that the need to escape from anxiety associated with the choice point interfered with the elimination of the fixated response.

As applied to neurotic behavior in human beings the insights gained by animal studies serve to clarify the rationale of such procedures as reactivating a traumatic experience during the counseling interview. The original anxiety is touched off by verbal symbols under modified conditions of safety or security or the availability of help due to the presence of the therapist. In terms of Farber's experiment it might be said that response fixation is less likely to persist if tension reduction is associated with "critical points" in the patient's life history. More effective adjustments can be worked out once anxiety ceases to arouse a rigidly stereotyped escape reaction.

The Role of Insight. The preceding allusions to conditioning serve to show how learning theory can be applied to psychotherapy. Theories other than conditioning are, of course, equally applicable. In fact, if conditioning be viewed as essentially a peripheral rather than a central theory, it is highly probable that some of the rival or alternative theories will be more applicable. For present expository purposes there is no need to defend one at the expense of the other. Shoben (1948, p. 136), however, seems to be of the opinion that "a learning-theory interpretation of psychotherapy" is more

amenable to a Hullian formulation than to one developed in line with Tolman's views. One may question this in terms of the great importance psychotherapists attach to the need for giving the patient "insight" into his problem. The fact that Tolman's systematic position is closer to Gestalt theory would seem to render his theory more capable of doing justice to the problem of insight

Insight has to do with the perception of relevant relations between means and ends, cause and effect, and motives and behavior. Much of psychotherapy may be viewed as a technique for rendering such perception possible in case its absence is taken to be a hindrance to the patient's recovery or adjustment In short, the patient *learns* to see his difficulties in a different perspective Whether this requires a genetic approach for effective treatment, as the Freudians and their followers hold, has been questioned by some They wonder whether the acquisition of insight necessarily calls for exploration of the historical antecedents of today's attitudes and difficulties

Kurt Lewin's topological and vector psychology, as Frank (1944) has shown, constitutes an *ahistorical* or nongenetic approach to adjustment problems Such problems, although rooted in the past, are always contemporary Under the circumstances much of the past may no longer have a sufficiently direct bearing on the present. One can gain an understanding of or insight into the meaning of a word like *pecuniary* without tracing it back to its root meaning of *cattle*. The decimal system can be learned without first being taught its historical linkage with primitive finger counting. Similarly, it might be argued, stage-fright can be conquered even though no effort is made to study the origin of the initial attack of such a fear. Inefficient habits can be replaced by more efficient ones despite failure to ferret out the beginnings of the former. A man can decide on the wisdom of divorce from a shrewish wife without reviewing his entire psychosexual history. Only that portion of the history continuing to influence the present need be taken account of in the search for a solution.

In many respects the nondirective psychotherapy of Carl Rogers, although it owes much to the thinking of Freud and Rank, seems to be in agreement with this Lewinian emphasis on problem solving

in terms of the here and now. This does not mean that problems of motivation—both conscious and unconscious—are ignored in these approaches, but merely that the orbit of investigation is limited to dynamic factors operative in the present. Restriction of this kind, it is hoped by its advocates, will shorten the time needed for treatment or for gaining the insights required for the emergence of more efficient patterns of behavior.

Psychotherapy and the Functional Psychoses.—In the preceding paragraphs discussion of psychotherapy as a form of teaching and learning was elaborated in terms of neurotic symptoms. This restriction was solely a matter of expository convenience and not intended to suggest that this approach to psychotherapy had no application to psychotic conditions. In this hasty outline there is no room for detailed consideration of theories of any of the specific psychiatric syndromes. Nevertheless the discussion would be flagrantly incomplete without some attention being devoted to the major functional psychoses.

The conditions placed in this category are subject to the vicissitudes of psychiatric thinking. The Kraepelinian nosology has been replaced by the 1934 "Official Classification" referred to by many textbooks and easily accessible in the text by Rosanoff (1938, pp 967-985). A still more recent classification is the one prepared for Army psychiatrists during World War II.[16] According to this system there are three groups of disorders to be designated as "psychoses without known organic etiology" and hence presumably functional in nature. These are the schizophrenic, affective, and paranoid disorders, respectively.

Whether these conditions are altogether devoid of predisposing constitutional factors continues to be a subject of contemporary con-

[16] The Army classification has appeared in *Mental Hygiene* 1946, **30**, 456-476 as well as in *Jour. Ment. Sci.*, 1946, **92**, 425-441. It appeared originally as *United States Army Technical Bulletin No. 203*. Washington, D. C., United States Government Printing Office, October 19, 1945, Section 18. In general, it is a more dynamic classification than earlier ones and thus reflects current psychological theory more adequately. It has already influenced contemporary textbooks such as those by Cameron (1947) and Strecker, Ebaugh, and Ewald (1947).

troversy, but the evidence is too involved to be summarized here.[17] It is enough to point out that some specialists are inclined to interpret the seeming success of some of the newer methods of treatment as incompatible with an exclusively *functional* theory applied to these psychoses. The various pharmacological shock treatments are included among these. So are the techniques of electroshock and prefrontal lobotomy. Such chemical, electrical, and surgical treatment procedures hardly fall within the purview of psychotherapy. At least so it would seem upon casual consideration

Nevertheless not all psychiatrists have been willing to interpret the success of these procedures as evidence for the nonfunctional nature of the psychoses involved. The very diversity of the methods renders it difficult to attribute the successes to a single biochemical or a single set of structural factors. Even in speculative terms it has not yet been possible to work out a plausible organic or neurological rationale for them Their continued use is to be justified more on pragmatic grounds than on the basis of the kind of intelligent medical insight governing the treatment of simple fractures Furthermore, it is generally agreed that one of the chief results of shock treatment is to render the patient accessible to psychotherapy From this viewpoint the shock is a means to a therapeutic end rather than a therapeutic agent in its own right.

This need to follow shock treatment with a regime of psychotherapy, some would maintain, in itself demonstrates the role of functional factors in the etiology of these psychoses. It has also been argued by some that psychological factors may be operative during the course of shock treatment itself They actually refer to the "psychodynamics" involved in such treatment

Rennie (1943) has furnished a convenient digest of these psychodynamic theories In one of them, for example, shock treatment is likened to the outmoded techniques of arousing sudden fright in patients in the hope of shocking them out of their delusional pre-

[17] The relevant literature is vast For purposes of initial orientation the volume by Hoskins (1946) might be consulted The impressive genetic investigations by Kallmann (1946) should not be overlooked A less technical review of the evidence pertaining to these problems is supplied by Klein (1944, pp 149 156, 208f.)

occupations by forcing them to struggle for life. A somewhat similar interpretation is advanced by another psychiatrist who believes the shock to imply a death threat for the patient. The need to cope with such a threat is presumed to arouse the "instinct of self-preservation" with all that this implies for a more positive attitude toward the world. A more psychoanalytically disposed psychiatrist views shock treatment as one way of reducing the hold of a "tyrannical superego." Still another such psychiatrist believes affective processes are released by the shock. He refers to these as involving the "joy of rebirth" following the restoration of consciousness after being comatose. There are other theories stressing the probable import of the shock experience to the patient as more crucial than the metabolic or biophysical changes incident to the shock-inducing procedures.

It may also be that what Stone (1947), as a result of his study of electroshock, has called a "generalized alteration of cognitive functions, of which memory changes are only an integral part," may have something to do with a modification of the patient's orientation toward himself and his difficulties. The emergence of new patterns of perception and a change in outlook might well follow such alteration. Should this be the case, then even these more obviously "physical" techniques of therapy might be effective to the extent that they might pave the way for more effective *learning* on the part of the patient.

The importance of supplementing shock treatment with psychotherapy is generally acknowledged by contemporary psychiatrists. If the present analysis is sound, this means that even in the case of psychoses such psychotherapy is to be envisaged within the framework of learning theory. Support for this interpretation of psychotherapy is to be found in one of the last articles written by the psychiatrist and psychoanalyst, Harry Stack Sullivan. In this article Sullivan (1949, p. 9) ventures to define psychotherapy as "the inducing by chiefly verbal interchange of changes in the patient's living with significant others."

Is not the induction of such changes by such means a form of teaching? If so, then psychology has an important contribution to make by bringing this kind of teaching into line with learning

theory in general. This applies with particular force to the application of learning theory to affective attitudes and the life of feeling and emotion as it relates to problems of inner security, self-confidence, and other mental hygiene issues. By thus divesting psychotherapy of a mystical halo and subjecting it to the kind of research now taken for granted in other areas of learning there is a greater probability of substituting scientifically grounded techniques for the present uncertain fumbling.

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